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HUMAN ABILITIES[1]

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Some recent experiences in test construction and in the use of factor methods have shown the need of clarifying the basis of mental measurement by means of tests. It is time to take stock of the work of the last decade on the analysis of abilities in order to see better just where we are and what some of the next steps should be. Much fine work of this kind has issued from the hands of Garrett and his students at Columbia, Woodrow at the University of Illinois, as well as from the University of Chicago. The work preceding the past decade has already been given an organized presentation, for the most part, in Spearman's classical *Abilities of Man* (**32**). It will be my main purpose to see what fundamental abilities have been established with an acceptable degree of certitude, to attempt to see under what conditions the fundamental abilities will be exhibited, and to point out certain conditions that should be observed more fully in test construction and application.

Human abilities is a large and intricate subject, and there is space to touch upon only a few limited aspects of it. For example, I do not propose to discuss that perennial question of the relative influences of heredity and environment. Neither is it necessary in this connection to dwell at length upon the meaning of 'ability.' I shall use the term in a sense similar to Carr and Kingsbury's definition (**9**) as referring to the constitutional conditions of individuals for performing in some specified manner.

[1] This paper represents the body of a presidential address delivered at the fifteenth annual meeting of the Midwestern Psychological Association held at the University of Chicago, May 3-4, 1940. .

Another aspect of the subject that I do not venture to discuss is that of the mathematical basis of factorial methods. But since much of this paper has to do with the use of factorial methods, I shall try to make clear at the outset my biases with regard to factor theory. At this stage of things factorial, any choice of method or theory is largely a matter of prejudice, in view of the absence of any final proof which compels assent. Four years ago I expressed a definite preference for Thurstone's theory and method (**16**). Subsequent experiences have only served to entrench that preference. This method, as is now well known, involves the extraction of a small though sufficient number of centroid factors and the subsequent rotation of axes. To mention one reservation, it seems doubtful whether Thurstone's full criteria for what he calls 'simple structure' are often realized (**36**, p. 156). Lacking the necessary statistical criteria for accepting factor loadings as being zero, we cannot be sure whether or not a sufficient number of vanishing loadings are present in a factor problem. But it does appear that when we are dealing with abilities the criterion of a 'positive manifold' is nearly always possible and when the maximum number of zero or very small factor loadings is attained by rotation, tests of ability fall into definite clusters or along planes, a fact which indicates that fundamental abilities are truly differentiable. Other methods, as Thurstone has often maintained, either assume some particular kind of factor pattern at the beginning or else arrive at factors which have a small chance of being psychologically meaningful. When the term 'factor' is used in the remainder of this paper it refers to a *common* factor, *i.e.*, one shared by two or more different tests, unless otherwise specified. It does not necessarily imply uncorrelated factors, but in practice the majority of studies do seem to favor orthogonal systems.

A Short History of Test Theory

The psychologist's story of human abilities might well be given the subtitle 'from faculties to factors.' The road has been a long and rough one, both temporally and scientifically.

There are some, it is true, who believe that the shift from faculties to factors is a very short one when viewed from the logical standpoint (4). To maintain this conclusion, however, is to ignore the development of psychology as an empirical science and the application of operational modes of thought, both of which are so clearly exhibited in the factorial approach.

Modern psychology, in its theory of abilities, has wandered over much logical territory. On the one hand there are those who have held that abilities are very specific. Among the first of these was Galton, with his early tests of simple sensory and motor powers. Galton was not particularly concerned over the underlying organization of abilities. He simply took the more obvious sensory and motor performances that laid themselves open to operational isolation and quantitative description. The functional unities seemed apparent when they could be measured separately. It might have been better for the testers succeeding him, had they continued along the same course for a time, remaining close to an operational basis.

But such was not the case. The early German and French testers, reflecting their philosophical heritage, and being only one step removed from the faculty psychology of their progenitors, generally assumed that they were measuring important mental functions, such as perception, attention, memory, and reasoning. Tests could be and were designated according to the function they were supposed to measure; memory tests, association tests, or imagination tests, and the like. This mode of thinking was common even as late as 1915, when Whipple published his classical collection of tests (42). If the implicit assumptions underlying this type of theory were true, then we should find that tests of any one function correlate perfectly, or nearly so, with one another and zero, or nearly so, with tests of any other function. It must have been known almost from the first use of the correlation method with tests that this kind of correlational pattern did not occur. Yet the custom of designating tests in this manner persisted to a large extent.

Parallel with this course of things, there emerged the great and glamorous concept of intelligence. In its modern sense, the word came into general use first with the naturalists and biologists of the last century. Intelligence was set over against instinct. For bringing it into the lexicon of the psychologist, Binet probably is most responsible. Binet had done much with tests before receiving his famous commission so often mentioned in our textbooks. He then put his tests to work attempting with their aid to distinguish between the bright and the dull. Had he never been given this assignment one wonders what direction the test movement might have taken. A general distinction between the bright and the dull suggests immediately a single dimension of mind. A popular brightness-dullness distinction was thus given scientific status; a single concept was demanded and 'intelligence' became that concept. The tests became intelligence tests, whereas before they had been known simply as mental tests. After further study, of the dull and feebleminded in particular, Binet was ready for his famous three-point definition of intelligence. This opened a new era in which the logical exercise of defining intelligence became the free-for-all pastime. Scarcely anyone doubted that there was an important something going by the name of intelligence, but no one could define it satisfactorily. Some took the attitude that "It is there, all right, but we do not need to define it or know its real nature." Others took seriously the facetious definition that intelligence is the thing that intelligence tests test, without worrying about whether there was agreement as to which tests were tests of intelligence. Still others, cognizant of the high, medium, and even low correlations among tests of intelligence so-called, wisely decided that we must distinguish between a Binet IQ, a Pintner-Patterson IQ, and any other IQ as representing somewhat different kinds of intelligence. For still others the IQ as an absolute, real, something, lives on, though there is now some doubt about its remaining in its place when one's back is turned.

When factor theory and methods first came on the scene a whole generation ago, they did much to foster the idea of a

single master ability. Although Spearman has always been wary about identifying his universal *g* factor with the concept of intelligence, and properly so, yet those who believed in the master ability were quite willing to accept Spearman's work as supporting their theory. Spearman added support to the cause by insisting that the Binet scale, the model for all intelligence tests, was constructed on the basis of his *g*-factor theory. Spearman and his followers were finally forced to recognize other common factors in addition to *g*. But those additional factors, known as group factors, were grudgingly admitted and their scope and importance minimized. Other investigators, beginning with Kelley (**23**), have found the group factors of greater importance, and in more recent years most investigations, though finding abundant evidence of extensive common factors, have often failed to find a *g* factor in their results. We shall return to the discussion of the *g* factor later, but will turn our attention for a moment to the general meaning of factors.

The Nature of Common Factors

What is the real nature of the factors discovered by the most elaborate of statistical techniques? The full discussion of this question would occupy a good-sized paper in itself. We can only note some of the answers at this time. At the one extreme, factors are regarded as mere statistical fictions. Some investigators, for example, finding that factors and factor loadings in the same tests tend to change under varying conditions, adopt the conclusion that there is nothing real or even constant underlying a factor pattern. The pattern, they say, is purely a mathematical one (**3**, **4**). Such investigators are looking for absolutes, which may be found somewhere in this universe but certainly cannot be found in the manner apparently expected among human abilities.

Thomson bids all factorists to reconcile their differences in theory by accepting his invitation to admit that factors are "only statistical coefficients, possibly without any more 'reality' than an average, or an index of the cost of living, or a standard deviation, or a correlation coefficient" (**34**, p. 42).

This statement fails to discriminate between factors and factor loadings. Carried to its logical conclusion it would affirm the futility of all statistical and mathematical procedures. An average is usually a description of something and so is an index of living and a correlation coefficient. Let us not overlook the real function of mathematics and statistics in science. Mathematics, including statistics, is in reality a language. It is used by scientists to describe their phenomena, in terms that are precise, economical, and subject to the processes of deduction. Computational processes, in the absence of computational errors, cannot in and of themselves produce so-called statistical artifacts. Such outcomes are the result of erroneous application of the mathematical vocabulary at the beginning. Here scientists always take a chance. This chance depends upon whether or not the scientific problem has been properly rationalized, including the proper assumptions. To say, then, that a factor discovered by the computational processes of factor method is nothing more than a 'statistical coefficient' is to belie the possibility of applying the language of mathematics to real phenomena and coming out with valid inferences about the nature of those phenomena.

There are other factorists, almost at the opposite extreme, who regard factors as real, basic, functional unities. The term 'primary abilities' implies such a conception. Without going into what the term 'functional unity' may mean, in detail, an illustration or two may suffice. Theories of color vision have traditionally assumed a small number of color processes which by occurring in various combinations give us all the variety of color experiences we have. The theories also tell us to look for the same number of structures or chemicals, in other words, organs which perform those primary color functions. To cite another example in the same sensory field, it is possible that there are in addition to the devices for seeing lights and colors, other mechanisms for seeing shapes and movement. What the underlying engines for these processes are we do not know. The evidence for the separability of the elemental functions we are now considering is

largely experimental and pathological in origin. If there are consistent individual differences in the efficiency of each one of these primary functions, and if tests can be set up in a way that will discriminate among individuals who are high and low in these functions, then factor methods should reveal their separateness, unless they are perfectly correlated, or nearly so, even though no test is a pure indicator of individual differences in any one of them. By analogy the situation should be essentially the same with respect to the factors discovered by comparable procedures in the more usual mental tests. For any factor isolated statistically one might then expect to find some underlying structure or engine correlated with it. The engine may or may not be localized; it may be some essential tissue without which the function cannot exist in any amount, or it may be some chemical property, or some dynamic property of nervous tissue in widespread areas of the brain. It is my view that though many factors do stand for such functional units with their structural bases in the individual, others may signify variables of other kinds, having their unitary reality in the form of biological or social properties of individuals or of groups.

It is entirely too early to specify or even to suggest with any degree of confidence the underlying organ for any one of the factors now recognized. It is likewise too early to say much concerning the hereditary basis for the factors and to what extent they are subject to improvement with training, either formal or informal. It is possible that some of the factors that seem unitary now will later yield to further analysis. To call the factors primary abilities without further proof of their elemental nature is to speak only in relative terms and as a matter of convenience. Considerable experimental design coupled with factor procedures is required to bring out in clearer focus the qualitative aspects of the isolated ability. In many cases the factor operation in the individual may be very obscure and not revealed by the more obvious similarities in the tests which are loaded with it. Every factor isolated is properly regarded as a hypothesis, subject to further verification and study.

A third common conception of factors is the sampling theory so long championed by Thomson in particular. According to this conception, the elemental human abilities are almost infinite in number and exist in the form of 'bonds,' associated on the physiological side with neurone arcs or synapses, or something structurally as elemental (**34**, p. 271). Spearman's *g* is conceived as the sum-total of all the bonds. When tests in a battery sample all of the bonds or even the great majority of them, the single common factor comes into prominence in the analysis. When only more scattered segments of the bonds are sampled by means of tests, common factors of less scope appear in subpools of tests (**34**, p. 283). Of this it can be said that when certain subpools of tests appear again and again in varied batteries of tests and with varied groups of subjects, there are some excellent underlying reasons for that clustering; it cannot be thought entirely fortuitous. In other words, there are underlying unities present. Even though those unities may rest upon combinations of functional bonds, they possess reality in much the same sense as a chemical compound. Carbon, hydrogen, and oxygen exist as real elements, but no less real are the hydrocarbon compounds that are composed of them. If it is admitted that a common factor is a compound made up of elements, as the sampling theory demands, then it must also be recognized that the compound is a relatively stable one. The only remaining basis for debate between Thomson and other factorists who believe in the unitary reality of factors would be essentially on the question of how stable the compounds are.

In passing it may be pointed out that the statistician's conception of correlated phenomena in general, and even of measured phenomena, is that they are composed of large numbers of independent elements which like coins have equal probabilities of functioning or not functioning in a given manner. Most people who apply statistical methods do so without making such assumptions about the phenomena under inspection. Their realities are more global unities and variables. Their statistical language applies merely *as if* the

atomistic conceptions like those of sampling theory did obtain. There is no good reason for regarding the statistician's hypothetical atoms as the real psychological phenomena and of doubting the larger, more apparent unities. Factors are usually real, they are global functions or variables and not fictions composed of questionable atoms.

What Factors Have Been Discovered?

What factors, with their tentative names and descriptions, have been demonstrated to date? A number have been verified with convincing regularity and others are still on the doubtful list. The question naturally applies to the first common factor of them all, Spearman's *g*. This factor was said to be universal in the sense that every test of human ability has some loading in it, at least all tests involving cognitive processes. The situation with respect to a crucial test of its universality has been an enigma. Unless all known tests could be shown to have loadings with the same factor, its universality would still be in doubt. Factor procedures which assume a *g* factor to be present and which proceed on that basis to extract factors will be almost certain to find it. When a table of intercorrelations, or a correlation matrix, contains nothing but positive coefficients, a pattern containing a *g* factor can always be found, but usually other patterns without a *g* factor would fit just as well mathematically. What the impartial investigator wants is a procedure that will permit the true factor pattern to emerge no matter what it may be like, with *g* or without. Now it has been supposed that Thurstone's centroid method, with subsequent rotation of axes, is unfitted to discover a *g* factor by virtue of its very assumptions. His criterion for the correct and meaningful position of the references axes is when simple structure is attained. As one of the requirements for simple structure he has specified that there should be at least one zero loading in every test. A *g* factor requires nonzero loadings in all tests. The two conditions, Spearman's *g* and Thurstone's simple structure, could not be more flatly contradictory.

The solution to this apparent impasse is that not all of

Thurstone's requirements for a simple structure can always be met. Thurstone seems to acquiesce to this in practice (**40**). Of all the criteria for the positions of meaningful axes, the demand for positive factor loadings throughout is perhaps the most compelling. The demand for a maximum number of zero loadings probably has the next claim. When these demands are satisfied there may still be a *g* factor. Several investigations have now reported a *g* factor when Thurstone's centroid procedures have been used. A notable example is that of Wright (**50**) who analyzed a large number of the tests in the first Stanford Revision of the Binet scale. She interpreted the universal factor in her battery of tests, not as Spearman's *g* but as a general maturation factor. Although her population had a narrow range of about one year chronologically, the range of mental ages was approximately eight years. Thurstone has very recently given a rationalization of the problem of mental growth which supports Wright's interpretation of her *g* factor (**40**, p. 206).

Most striking evidence that a *g* factor cannot escape a search by the centroid method is offered by a recent experience of the writer. A factor pattern involving three common factors in sixteen tests was arbitrarily set up. One of the three factors had loadings in all sixteen tests, and to make the detection of this factor more difficult, the two smallest loadings were .2 and .3. The other two factors had zero loadings in five tests each. A total of ten zeros is about what one should expect in an overdetermined simple structure when there are three common factors. From the factor loadings, intercorrelations among the tests were computed. To these coefficients were added small positive or negative increments in order to introduce sampling errors such as would be expected among the correlations in a population of 200 individuals. Two different sets or errors were prepared, making two sets of correlations and hence two factor problems. The two problems were assigned to a class of four graduate students who were beginning their study of factor methods. They were told nothing of the nature of the factor pattern but simply to follow the usual centroid procedures of extraction and rotation.

In every case, the students found by using all known tests that three factors were sufficient. Rotation of axes was accomplished both in planes, two axes at a time, and also on a sphere three at a time, until all loadings were positive and the number of zeros maximized. Incidentally, the best student's final factor matrix was so close to the original one that the standard error of the discrepancies for the 48 loadings was only .074, a value that is comparable with the standard error of the error increments which were added to the original correlations. In other words, the factor loadings were as reliable as a group as were the original correlation coefficients. The crucial problem, however, was to see whether the *g* factor would emerge. For every student it did so. Applying Thomson's test of the extensiveness of the most general factor (**34**, p. 263) it was definitely shown that there must be a *g* factor. These findings are of course only a straw in the wind, for the arbitrary selection of some other factor pattern with which to start might give different results. But it shows, along with other studies, an example of which has been cited (**50**), that Spearman's *g* or any *g* is not necessarily bound to escape the grasp of Thurstone's instruments of analysis.

Before mentioning other common factors it is necessary to say something concerning the identification and naming of the segregated abilities. The nature of a factor is surmised from the tests that are loaded most with it, and by tests that have significant loadings in contrast with other tests that have no loadings in it. There are three aspects of tests to which attention may be paid when looking for underlying identities in them. The three aspects are (1) material, (2) formal, and (3) functional. The first two are often recognized but the third is frequently overlooked. The material of the test may be words or closely related symbols; it may be forms, figures, numbers, or pictures of objects. In the mode of presentation the items may be in the form of analogies, paired associates, or series, or they may require an assembly of elements or the completion of a whole. Both the material and the formal aspects of the items have to do with the mechanics of the test itself. They are independent of the person

tested. Not so with respect to the third aspect of a test. The functional aspect of a test has to do with what it makes the testee actually do. In order to determine the functional aspect of a test item, we must make a job analysis of it. The factor analysis itself establishes the common functions in a battery of tests. But the psychologist's job analysis, using all possible aids including an experimental choice of tests and the facts derived from previous factor analysis, is necessary in order to bring into focus the essence of a factor. It would not be entirely without point to urge some direct observations of a testee at work on a test together with his introspective reports to provide additional information.

Smith (**31**) found a few years ago that tests seemed to cluster more readily according to their material content than they do according to their formal character. It is true that our three most frequently verified factors, verbal, numerical, and spatial, divide tests into groups along the material lines of words, numbers, and spatial forms. But it is probably what the testees have to do with words, numbers, and forms that is the common function in each case more than the nature of the material. With few exceptions, wherever those materials are involved the same fundamental operations are brought into play. There is good reason to believe, however, that the three factors may have loadings in tests *not* composed of the usual materials. That is to say, some tests not composed of words may have loadings in the verbal factor, and tests not composed of spatial forms may be loaded with the spatial factor. I have recently been experimenting with a test presented entirely verbally but requiring visualization or at least some kind of spatial thinking in order to solve the problems in the items. To a large extent, it is true, the material and formal aspects of a test condition the functions that are most needed, but they are certainly not the only determiners. In fact, many determiners of what abilities are important in a test depend upon the individual taking the test and his relation to the test, as will be illustrated further on.

It has already been suggested that the verbal, numerical,

and spatial factors have been confirmed over and over again. Their existence cannot be questioned though their exact natures are still in doubt. Another unitary ability that has been verified more than once is a memory factor (**1**, **2**, **6**, **8**). There is some suggestion in the work of Carlson (**8**) that the memory factor may be broken down into more elemental abilities or that there may be additional memory factors. A factor called speed of perception was discovered by Thurstone and has been verified by him (**37**) and by Harrell (**18**). There is one factor, perhaps more than one, which enters into tests where the grasping of relationships is important. This ability Thurstone has identified as 'induction.' The contours of this area of ability are as yet rather indistinct. A companion factor, which is found particularly in syllogism tests, can well be called 'deduction' or reasoning ability. This one should have been expected from classical psychology, but it has been one of the least conspicuous, perhaps because our tests infrequently demand the formation of inferences. I have a growing impression that specific abilities are relatively more important in reasoning tests than in many others, and that therefore high loadings with the deductive factor are not to be generally expected. Reasoning seems to exhibit at the symbolic level of activity many of the same characteristics in parallel with those found in sensori-motor learning, for example, the phenomena of trial and error and of insight (**17**, p. 445). If many kinds of reasoning tasks were brought into a factor problem it might be possible to demonstrate a number of subclusters each representing some well-learned type of skill with a moderate range of transfer. One of my students is now at work on the problem of factors in reasoning tests.

A factor of 'fluency' was reported by Thurstone (**38**) and was named by him 'verbal fluency' because in his study it was prominent in tests involving rapid verbal responses. This factor has been verified by Thornton (**35**), also in verbal tests. There is some probability, however, that this factor is not confined to verbal tests, since scores in the rate of fluctuation of the outline cube and productivity in an inkblot test bear some correlation with it (**12**). A common factor identified as

'attention' has been suggested from time to time (**25**), most recently by Woodrow (**46**), Richards and Nelson report a common alertness factor in tests of infants six months of age and which at the 12- and 18-month levels seemed to merit the title of 'alertness-verbal' (**26**).

It should be remarked that even though some fundamental ability is essential in performing a number of tests, or all of the tests, in a battery, analysis may not reveal that ability as a factor. To illustrate, nearly all tests composed of visual material presuppose visual acuity as an essential condition. The reason that visual acuity does not emerge as a separate factor is that individual differences in scores do not reflect individual differences in acuity for the reason that all the individuals probably have at least the minimum essential acuity. Above this minimum, greater visual acuity is of no advantage to the testee. If the printed material were made smaller and smaller so that those of poor acuity lose in test score because of it in proportion to their deficiencies, then visual acuity would be at a premium and would undoubtedly show up as a primary ability. Similar reasoning applies to all tests and factors. Very few tests are so devised that differences in attention are of any serious consequence in the final scores, so attention is rarely verified as a common factor. Following this line of reasoning still further, it leads us to question seriously whether the tests now commonly known and used are sufficient to reveal all the important factors in human ability. We have, indeed, been rather definitely proscribed in our search for factors because our vision has been kept within the horizon of the present tests. We have done little, for example, to explore the area of creative talents. It is incumbent upon us to extend our horizon. The invention of tests in new areas is challenging but not an impossible assignment.

One area of testing that has interested many psychologists is that of motor performances. Buxton's recent preliminary analysis (**8**) tends to show that there are common motor factors. As Seashore (**29**) points out, however, intercorrelations among motor tests are notoriously low and clusters are limited

in scope. This fact forecasts a number of motor abilities of narrow range and tests with relatively low communalities. General factors of broad scope among motor abilities may include physical strength (**35**), agility, and steadiness or control (**8**, **30**). Other results suggest the highly interesting hypothesis that it is the pattern of muscular movements which is the key to communality among motor tests, rather than the involvement of particular groups of muscles or sensory equipment (**30**).

Two factors that long experience has led psychologists to expect are those of 'speed' and 'learning ability.' Both are still of very doubtful status. There is no single factor that seems to be common to time-limit tests and absent in non-time-limit tests such as might be expected if there were a universal mental speed factor. Speed, as we ordinarily think of it, seems to be rather specific to each test and since many tests involve it as a parameter, variances in speed are absorbed in the other common factors. Some results of Woodrow (**44**) tend to show that when other factor variances are relatively unimportant and the task is a simple one in which output per unit of time is paramount, a speed factor shows up. But this same speed factor is not a serious determiner of variance in scores in many tests and it may be confined to motor-speed tests.

As for learning ability, contrary to common observation, this also seems to be rather specific to the task itself. Every kind of task, or test, offers opportunity for its own kind of learning or learnings. Woodrow (**43**, **45**) has shown that when gain scores are analyzed along with initial and final scores, or when the factors in gain scores are estimated in other ways, there appear in gain scores no factors that were not held in common with either initial or final scores. Other studies lead to the conclusion that practice in tests does not augment the fundamental abilities but changes their relative importance in performing those tasks (**47**, **48**). Learning thus seems to result primarily in a change of methods of doing things and so a change in the relative importance of primary abilities in the learned tasks. These findings, along with others (**22**), should

be sufficient to do serious damage to the definition which identifies intelligence with learning ability. The impression that children do differ in general learning capacity in school may be due to a number of things. Most school learning is of a verbal nature and to the extent that the verbal factor is a condition of learning and to the extent that this is true in different subject matters, the impression of a single learning ability is apparent. Another thing involved is the fact that many school subjects draw upon more than one fundamental ability and so a child's inequalities may be obscured to common observation. Some differences in learning ability within the same child are noticed, of course, but even these may be swallowed up to some extent in a general halo effect.

Conditions Influencing Factor Patterns

We now examine briefly the various conditions that tend to alter the factor compositions of tests. It should be remembered that abilities are properties of individuals and not of tests. But whether or not any factor, for example, the number factor, is identical as to kind in all similar human beings and different only in quantitative variations is a question that is open to further study. It is most convenient at the present time to proceed on the hypothesis that the answer is in the affirmative. Factors are only secondarily properties of tests in so far as those tests reveal those abilities in individuals.

It must be admitted that many conditions have the power to alter the factorial picture in the same tests. Some of these conditions reside in the population tested—its heterogeneity, its age level, its position on learning curves for skills that will affect test scores, and the variability of the individual (**20**). Other conditions have to do with the nature of the test—its material and formal content, the kind of instructions, and the kind of score. Still other conditions have to do with the relation of the individuals to the test—the level of difficulty of the test, opportunities for learning (insight in particular) during the test, transfer of learning from an earlier test to a later one in the battery, and the possibilities for vicarious functioning of a more able function for

a less able one. Most of the last group of conditions would come under the general heading of the functional aspect of the test, or what it requires or permits the individual to do in order to help make a given score.

Factorists have recognized for a long time that the correlated population should be relatively homogeneous with respect to such variables as age, sex, race, cultural background, formal education, and specific training (**23**). Even at best our population must be a finite sampling, but the same thing is true of all statistical work with individual differences. Thomson has demonstrated mathematically the fact that the introduction of heterogeneity into the tested group may not only alter factor loadings but may also create a new factor (**34**, p. 194). The new factor stands a good chance of having a basis of truth, however, in that it may represent an important source of heterogeneity. The study of Wright, for example, exhibited a general factor of mental maturity, as she interpreted it, in a group of children heterogeneous as to mental age (**50**). The wise factorist will be on the lookout for unusual factors of this kind and will refer them to whatever known source seems most reasonable. Their occasional appearance should not be sufficient grounds for rejecting the entire method.

Garrett and his students have contributed a number of studies with subjects whose chronological-age levels have varied from group to group (**13**). One of the primary questions of these investigations has concerned the organization of abilities at different ages. In general, with advancing age the intercorrelations among tests decrease in size, but unevenly. The general consequence is that common factors increase in number and in degree of independence. Some cases of significant correlations between factors have been found with children who were advanced as far as grade four. There are two alternative hypotheses that have been offered to account for the results. One is that life begins with one, or at least a very few, undifferentiated abilities, which in consequence of the processes of growth become later differentiated (**13**). Another hypothesis would be that young children pos-

sess the same unitary abilities as their elders but bring them into use in greater combination and in a manner that defies statistical analysis (**38**, p. 87). The first hypothesis assumes that tests having comparable functional aspects have been employed at the different age levels; the second hypothesis assumes that genuinely comparable tests have not been used. It may be that with proper construction and selection of tests the real status of developing mental organization can be brought to light. As yet we have only some interesting leads on this question.

Several factorial studies have shown that the factor compositions of tests are altered after practice in those tests, notably the studies of Anastasi (**3**) and of Woodrow (**43**, **45**, **47**, **48**). Some factors grow in importance and others decline as practice goes on. This problem is related to an earlier one in testing which asked at what point during the practice curve for a test are the most reliable and valid scores obtained. If the validity of the test bears a close relation to its factor composition, and presumably it does, then such training experiments will tell us when the factor we wish most to measure by means of the test will be at its maximum. From these studies we also deduce a conclusion concerning the art of testing. Individuals usually come to a test with varying amounts of specific practice in that kind of task, and also varying amounts of practice in similar tasks which might yield help by transfer. Since the factor loading in a test changes with practice, it follows that the same factor loading obtained under usual test procedures does not apply alike to all individuals. The obtained loading, like any statistic, represents a most probable or average value about which individuals vary. In other words, there are individual differences in factor patterns as well as in other quantitative aspects. In order to guarantee that we are measuring the same factor in all individuals they should have had comparable amounts and kinds of practice which bear on the test result. This caution has been well appreciated by testers before, but it can be too easily forgotten. We cannot generalize from the factor studies thus far as to whether practice in tests actually alters the factors

in the individuals or whether it merely changes the functional aspect of the tests. It is possible that training has much to do with the growth of factors in general. It is difficult, however, to see how a relatively small amount of specific practice could have any marked or lasting effect upon the factors concerned. At least in connection with the verbal factor the evidence is that special training does not augment it but rather changes the manner of working at the task (**47**).

Formal practice produces changes in factor composition by altering the difficulty of test items. But there are other things that make items easy or difficult for an individual or for a population, and with this goes a change in the mode of functioning as the testee works on the items. Factors important for individual differences when items are easy lose in importance in the same kind of items when they are difficult. A most striking example of this was shown by an experience of two years ago. In a brief study of the matter, the Seashore test of pitch discrimination was employed. This test contains ten sets of items of ten each, with ten levels of difficulty ranging from items with differences of 30 cycles/sec. to those with 0.5 cycles/sec. With such homogeneity of *kind* of items, who could imagine that all parts were not equally entitled to the name of 'pitch-discrimination test'? Incidentally, in this last question lies one of our greatest sins in the whole test movement. Things going by the same name are taken to be identical without further question. After many years of mistakes we are freeing ourselves from this error as it applied to so-called intelligence tests. We still must free ourselves from its restricting influences whenever we name any test by a function of which we presuppose it to be a measure. It is doubtful whether a test should ever be named by reason of its functional aspect, even after its factorial content has been verified, for that content will probably vary according to the population examined with it. From this line of thinking, Seashore's test of which I am speaking might better be called simply a 'pitch-difference' test, naming it purely by its material and formal aspects.

The Seashore test had been administered to several hun-

dred university students, and from the result 300 papers were selected at random. These were scored for each of the ten parts separately. The intercorrelations were computed, tetrachoric r's being found by the graphic method. The two subtests of greatest difficulty were excluded from the analysis because their correlations with other subtests were very small, with some inclination toward negative r's, and because it was known that these parts were so difficult for the population that the judgments were almost pure guesswork and hence errors of measurement were too strongly represented in the variances. The centroid analysis showed definitely three common factors, with a doubtful fourth. One factor had its maximum loadings in the easiest subtests, a second was prominent among the subtests of medium difficulty, and the third among the most difficult subtests.

With test variables so limited in number and in kind, it is almost impossible to interpret these factors satisfactorily. One can only speculate. The first factor, which decreased in importance with the difficulty of the items, might be regarded as an attention factor. Failure to judge an easy item correctly could readily be due to a lapse of attention, or laxity of alertness of the testee. If any one of these three factors is a pure pitch-discrimination ability, it is most likely the second one. It is common experience that items of medium difficulty are the most discriminating or diagnostic for the variable being tested. The nature of the third factor is less easy to guess. Increasing in importance with difficulty, it is some function that aids individuals to obtain a good score among the difficult subtests but not among the easy ones. There are a number of implications here for testing that should be followed up but we shall not do so here.

Very little attention, except incidentally, has been paid to the kind of score chosen for a test as a condition of its factor composition. Scores are based upon some quantitative aspect of a performance—time, errors, rate, level of difficulty, gains, ratios, and the like. Although factorists frequently take the kind of score into account when attempting to interpret new factors, this is not always the case. A given kind of score is

usually adopted for a test because of convenience, or because of logical considerations. There is no guarantee that all the quantitative aspects of a performance are linearly related or are expressions of the same factor variances. In factor studies it is of great importance to consider the kind of scoring as such, and also the kind of scoring in combination with the variable of difficulty and with whether or not the test is given with a time limit or without.

Other conditions which are recognized but about which we know very little in factor investigations have to do with learning during a single test or during a battery of tests, and the possibility of vicarious functioning. There is no doubt that in many tests there is much opportunity for the testee to change his method of work to his advantage or disadvantage during the course of the test. Instruct him as carefully as we will, his set departs from that which we intended for him. For him the relative importance of the factors shifts. The same kind of thing may occur in other individuals, and the shifts of method may be in the same direction or in various directions. In some tests the instructions and the kind of items force the testee to do specific kinds of things and any departure from these procedures would be almost impossible. The relative purity and constancy of tests as measuring instruments depend upon accomplishing this result. Vicarious functioning should be controlled or recognized when we are attempting an anlysis, but it is also important for us to determine when and where vicarious functioning is successful and whether there are any laws concerning interchangeable unitary abilities. This constitutes a group of factor studies in itself, the results of which would be highly significant for vocational guidance and selection.

Some Examples of Functional Aspects of Tests

I wish to make more concrete some of the things just described by reference to specific test items. The first illustrations will be in terms of a very common kind of item appearing in the number-series test. In this kind of test the material and formal aspects are the same throughout. Anyone who

will take the time to examine the items in an ordinary number-series test in order to see what a normal individual might do with each item in his attempts to find the right answer, however, will be amazed at the ambiguity of the processes that might be demanded of a testee. In other words, the functional aspect varies not only from test to test but from item to item. Consider first a very simple case:

17 20 23 26 29 32 35 *38*

This is typical of many series which are based upon the most obvious principle of a constant increment, either positive or negative. The testee need not verbalize the general principle, of course, but in addition to grasping the principle of 'add 3,' he generalizes to some extent, if he has not already been taught the generalization with the instructions and practice exercises, and so later he expects similar items. Another simple principle in number series is the constant multiplier, which may be either greater or less than 1:

16 8 4 2 1 1/2 1/4 *1/8*

Other items soon introduce to the testee cases of alternation of increments and multipliers and if he generalizes sufficiently he is thereafter on the lookout for simple alternations. Insightful learning has thus occurred, changing to some extent what he has to do with subsequent items. Other kinds of items are more ambiguous in the possibilities that they offer, for example:

98 87 76 65 54 43 32 *21*

This series may be taken as having an increment of −11, a decrement of +11, two decrements—one in the first digit and one in the second—or it may be remarked by some individuals that the last digit of each number becomes the first of the next, the second digit in each case being one less than the first. A mathematical friend informs me that for every series there are an infinite number of ways in which it can be conceived. Practically, for nonmathematical testees the number of ways is decidedly more restricted. At any rate, the

very fact of some ambiguity in the responses to such items may have its effects upon the factor composition of the test.

The factors most important in number-series tests are the number and induction factors. To a much smaller extent the factors of perceptual speed and of deduction may enter at times. The number factor is more important when the grasping of the principle and the selection of the answer depend much upon accurate computation. The inductive factor is more prominent when the computations required are very simple and computational slips are rare. The fact that most of the principles in number series are essentially numerical principles probably precludes an elimination of the number factor entirely. If a test is very much confined to a few kinds of principles, for example, if all of them are simple increments and decrements, the number factor should again come into greater prominence and perhaps the perceptual-speed factor. On the other hand, if the principle changes very frequently and varies radically in kind from item to item, the induction factor should gain in importance. From this it would follow that a number-series test that is optimal for measuring inductive ability should change the kind of principle, at least slightly, with every new item. This fact also suggests that the inductive factor is essentially a learning process which has to do with the formation of new mental constructs. The kinds of principles could become relatively elaborate as in the following:

1 3 7 15 31 63 127 *255*

in which the change can be given as $(2N_0 + 1)$ where N_0 is each number in turn and the $f(N_0)$ expresses the change. Another radically different principle appears in the series:

1 3 5 7 11 13 17 *19*

which is composed of the first seven odd prime numbers in ascending sequence and calls for the eighth. It is very likely that we are missing an opportunity for measuring the higher reaches of intellect in this test and in many others by not including items of this altitude. However, in these particular

illustrations the influence of mathematical training would begin to appear. It might come out as a contributor to specific variance, or as a new common factor if other tests in the battery were influenced by the same condition.

Let us take a few more examples from the figure-analogies test. In this kind of test at least five unitary abilities are susceptible of measurement: Perceptual speed, spatial, verbal, deduction, and induction. Which factors will emerge with relatively high or low loadings will depend upon the construction of the items. Perceptual speed will probably be most in evidence in a timed test when:

(1) The figures are small or detailed (see figure, p. 391; Item 1).
(2) The figures are relatively complex (Item 2).
(3) The change from A to B is slight (Item 3).
(4) When in a multiple-choice response the correct figure is hard to discriminate from among its distractors, the rule or principle having been easy to apprehend (Item 4).

The factor of spatial thinking will be most important when:

(1) The principle of change depends upon a rotation (Item 5);
(2) The principle depends upon inversions (Item 6); or
(3) Upon the rearrangement of parts (Item 7).

The verbal factor might be expected to enter to some extent when the rule is complex or is easily and naturally verbalized, as in Item 8. This will also depend upon the individual, however, and his habits of depending or not depending upon verbalized methods. Here is a good example of vicarious functioning of unitary abilities.

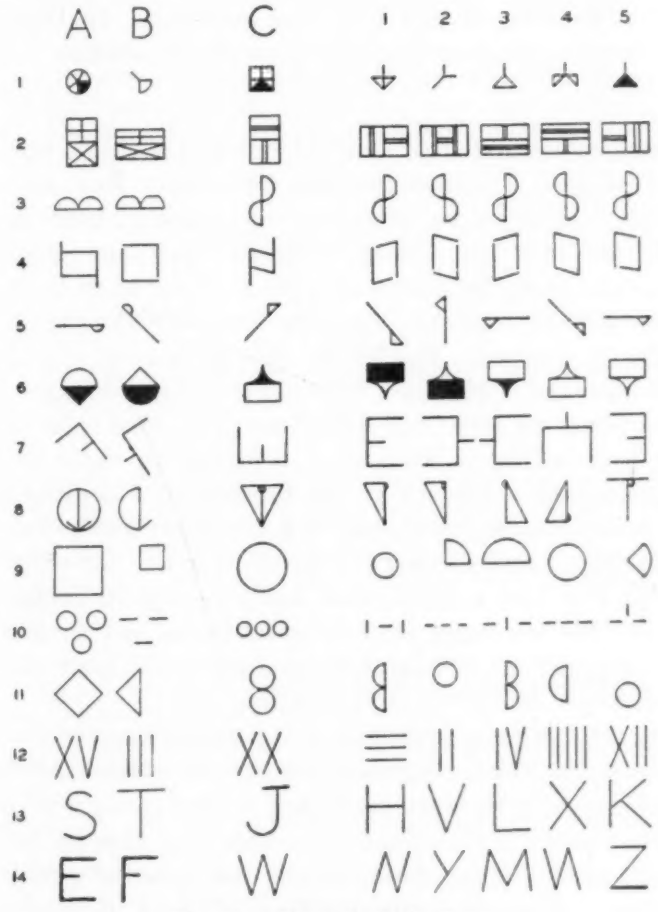
Deduction may be expected to enter most when:

(1) There are fine distinctions to be made among several alternative rules all of which come close to fitting (Item 9). The relation in Item 9 could be conceived as one of size and change of position or as a whole-part relationship. The responses do not provide for the first rule but do for the second, therefore the second rule is correct.

(2) There are fine distinctions to be made among alterna-

tive answers (Item 10). Responses 2 and 4 satisfy the rule as to arrangement, but 4 is preferred on the basis of size of lines and spacing between them. Therefore 4 is correct.

Finally, the inductive process, for which the test was primarily constructed, will have its greatest opportunity to reveal itself when:



Items in a Figure-Analogies Test

(1) All other factors are at a minimum (Item 11), that is to say, when the figures are large and lacking in detail, no rotation or rearrangement of parts is required, and discriminations are simple.

(2) The minimum essential for a correct response requires a correct apprehension of the rule, as in Item 11.

(3) The same kind of principle is not repeated; a frequent change in kind of rule, as in the number-series test, demands new inductions and not simply the reproduction of already learned ones (Items 12 and 13). In Item 12 a number relationship is suddenly inserted when none of this kind had preceded. In Item 13 a letter sequence appears as a novel relation for this test.

The moderate number of kinds of rule and their various combinations give us much latitude for this. But when the principle is changed too often and too radically, there is much chance of some new ability being brought into importance (Item 14). Here the testee may be set because of Item 13 to look for a letter sequence but must shake off the previous set in order to succeed. The ability to shift mental sets quickly may some time appear as a factor, but has not done so as yet, unless it is to be identified with fluency or some other ability.

In conclusion, if we are to continue to make effective progress in the discovery of new fundamental abilities and in their measurement, we should look to a more analytical study of individuals and of tests. And this implies not only factor analysis but also experimental analysis (15) in combination with it. We need new tests of novel kinds, but we also need keen observations of human mental processes, particularly in the sphere of the symbolic activities.

Factor methods have now well justified themselves. The uncertainties which plagued earlier factor studies are one by one giving way to more assurance in the validity of results. Experience teaches us their limitations and how better to interpret our findings. Some of the fundamental problems of individual differences are yielding at least to preliminary solutions. When the nomothetic ways in which human individuals differ one from another are better known, we shall be in a more reasonable position to attempt the solution of the causes of those differences, and to answer the questions con-

cerning growth and decline of human powers, sex and race differences, the contributions of heredity and environment, and concerning many vocational and larger social problems. Toward these ends our study of human abilities has always been irresistibily drawn.

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THE NATURE OF MENTAL DEFICIENCY[1]

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Mental deficiency commands scientific attention as a form of human deviation which illuminates some of the basic theories in biology, physiology, psychology, education, and the social sciences generally. This condition reflects 'natural experiments' in human morphology which make possible investigations on the nature of man and his social modification.

Such research does more than clarify the related practical problems of education and public welfare. The segregation of mentally deficient persons in public and private institutions, as well as their distribution in the community generally, affords unusual opportunities for exploring many postulates in psychobiology. The time seems opportune for encouraging well-grounded attitudes and sound conceptions regarding this condition in order that the results of scientific exploration may prove helpful rather than misleading.

Historical

Scientific interest in the feeble-minded dates from Itard's treatment of the Savage of Aveyron at the beginning of the nineteenth century (**12**). This 'wild boy' afforded a critical test of the philosophical doctrines of the day. Pinel contended that Victor was congenitally defective and that neither treatment nor training could make him normal. Itard believed that this *homme sauvage* was a product of environmental deprivation and was capable of attaining normal status through educational stimulation. The old problem of nature and nurture was thus put to critical test in an extreme case of mental deficiency employing a clinical method of experimen-

[1] Presented at the Graduate Seminar in Psychology, Princeton University, March 18, 1940.

tation. Earlier experiments of a cruder sort are reviewed by Briffault (**4**, Chapter I).

Itard's experiment revealed that Pinel was right, but also proved that environment and training do modify behavior. Then as now the problem was reducible to one of 'the limits within which' nurture may influence nature. Except for Itard's painstaking efforts, Victor might have lived on as a wild animal, precariously sustaining life at the brute level of subsistence in an environment devoid of human contacts. His training produced some rudimentary attainments in language, self-help, and social participation. By maximum capitalization of limited endowment through physiological and social stimulation an apparently high-grade idiot was transformed to a low-grade imbecile.

This experiment like those of Seguin which followed (**19**) emphasized the relation of learning to the constitutional limitations of the organism. Social opportunity can only foster the personal capitalization of congenital aptitude; it cannot *create* talent that is genetically lacking. Education can only exploit congenital endowment; it cannot overcome inherent deficiencies. Certain achievements may be inculcated as substitutes for original shortcomings, but can never be more than crutches to ameliorate organic weakness. Growth and development are *relatively* and finitely interdependent.

During the period between approximately 1850 and 1900 institutional care of the feeble-minded had its greatest initial impetus.[2] During this period mental deficiency was considered a disease, a pathological variation from the norm of human development.

About 1900 the British Royal Commission (**17**) defined mental deficiency in terms of a four-fold criterion, namely, (*a*) social incompetence, due to (*b*) mental incompetence, resulting from (*c*) arrested development of (*d*) constitutional (chiefly hereditary) origin. Although recognizable from certain symptoms in childhood and youth the condition was

[2] See (**1**) for an early history and a review of the problems of definition, classification and etiology.

conceived as obtaining at maturity and as essentially incurable though educationally and socially ameliorable.

This position has been reaffirmed by subsequent British commissions and has had its best and most recent expression in the report of the Wood Committee (**24**). This point of view was incorporated into British law for purposes of social and educational practise. It was also adopted by Tredgold in his classical text-book treatment (**21**). It is to be noted that in British terminology, and in common American usage, the term mental deficiency (also amentia) is synonymous with feeble-mindedness as a generic term. The latter term is still employed in England for the highest grade of mental deficiency, now termed moron in American usage.

In the first decade of the twentieth century, Binet and Simon examined the concept of mental deficiency with a view to improving its clinical determination for educational purposes (**2**, **3**). They acknowledged the fundamental inability of the feeble-minded to maintain themselves socially without supervision and assistance. They also emphasized the constitutional preconditions and the educational manifestations of feeble-mindedness. They urged the essential *mental* incompetence of the feeble-minded as the basic manifestation and from this point of departure developed their now famous measuring scale. Their work led to a wider recognition of feeble-mindedness among children of school age. Their mental age method has since largely overshadowed comprehensive clinical diagnosis of this condition.

It is regrettable that Binet and Simon did not more emphatically elaborate the acknowledged social manifestations of mental deficiency. And it is most unfortunate that their emphasis on the mental and scholastic development of the feeble-minded should have led others since them to ignore the social and physiological preconditions. Yet in the prolific writings of Binet, with Simon and others during the period of 1900 to 1910, one finds a wide range and variety of explorations on the social and somatic as well as the educational and psychological (other than intelligence) characteristics of the feeble-minded. The mental-age epitome of these results has

since proved as restrictive in the field of mental deficiency as it at first proved stimulating. One must regret that so many have sacrificed the patrimony of psychology at the altar of this one idol of psychometry. Had Binet lived longer it is likely that he would have been among the most vigorous to protest such stultification of thought and practise.

In 1910 the American Association for the Study of the Feeble-Minded adopted a classification of the feeble-minded based on Binet mental age (**18**). Although this was designed only to indicate the grades of deficiency *after* the feeble-mindedness was established from other evidence, it soon led to the indiscriminate use of mental age as the principal criterion of mental deficiency. More seriously, this classification innocently led many to employ the upper mental age limit of feeble-mindedness (about twelve years) as the lower limit of normality, a logical error of the obverse which has not since been eradicated.

In promulgating the 1916 Stanford revision of the Binet scale Terman carried the mental age concept of mental deficiency to more serious limits through the use of the IQ (**20**). After acknowledging (p. 80) the fundamental social concept and the fact of overlapping in IQ at the borderline of deficiency and normality, he concludes without validation that "All who test below 70 IQ by the Stanford revision of the Binet-Simon scale should be considered feeble-minded." This position became explicit in the legal definition of feeble-mindedness in California by permitting the alternative substitution of an intelligence level of less than thirteen years for the criterion of social insufficiency (**23**).

If social adequacy is the essential criterion of mental deficiency, the mental criterion must be validated against it. But if the social criterion is vague, then the mental-age or IQ validation must be equally dubious. Moreover, the mental criterion serves to *explain* rather than to replace the social criterion. As noted later, the accurate measurement of social competence by means of the Vineland Social Maturity Scale (**6**) does not alter this problem but only clarifies it.

An IQ classification of the degrees of mental deficiency

was adopted by the American Association for the Study of the Feeble-Minded in 1920 to supplement the mental age standards (**11**). Like the 1910 mental age classification, this 'graded' the feeble-minded as idiot, imbecile, and moron *after* the feeble-mindedness was determined by other means. Designed originally to simplify statistical compilation of institutional populations, especially with reference to children, it soon became a substitute for clinical diagnosis. This practise overlooks the symptom-complex nature of the several degrees of mental deficiency (**9**), and ignores the inconstant trends of IQ's among individual feeble-minded subjects as age advances.

The 1930 White House Conference committee (**13**) presented substantially the following argument:

(*a*) Feeble-mindedness is defined as social incompetence due to arrested mental development resulting from organic causes.

(*b*) The moron degree of feeble-mindedness extends from adult Binet MA 8 to 12 years. The corresponding Binet IQ range is from 50 to 75 employing the 16-year adult divisor, or from 57 to 86 using the 14-year divisor.

(*c*) Dull-normality is defined as intellectual subnormality not resulting in social incompetence. The Binet MA and IQ range for this condition is the same as that for moron feeble-mindedness.

(*d*) In the professional literature and in law mental deficiency has long and widely been used as synonymous with feeble-mindedness. But in recent psychological literature and practise mental deficiency has been considered as adequately determined by a 1916 Stanford Binet IQ below 70 (16-year adult divisor) unsupported by other evidence although the desirability of additional clinical evidence is acknowledged.

(*e*) Therefore mental deficiency may tentatively be defined to include both feeble-mindedness (low IQ with social incompetence) and intellectual subnormality (low IQ without social incompetence). Low IQ is considered as below 85 by 1916 Stanford Binet using the 14-year divisor.

The committee emphasized the importance of situational factors in borderline cases. They found it difficult consistently to reformulate standard programs for the social control of the feeble-minded mentally deficient in such a way as not to include the dull-normals. Editorial consistency was easier to achieve than ideational harmony, for the report of this committee except for the standpoint on definition is in fact confined to those mentally deficient who are feeble-minded.

A differential consideration of intellectual subnormality and clinical feeble-mindedness as separate categories of mental deficiency has yet to be accomplished. Correlations among various intelligence tests, and between intelligence tests compared with other mental qualities, and with social and educational success, while relatively high are not high enough for individual prediction. Other evidence has disclosed individual inconsistencies in mental test ratings according to the kind of performance required, as for example linguistic *versus* non-linguistic tests. These disparities have led some investigators toward battery type of mental measurement, have induced others to challenge the differentiating criteria, have persuaded many to define mental deficiency in such a way as to include normality with feeble-mindedness, but have encouraged few to seek new methods of realistic attack on the problem by other than intelligence test approaches. The conversion of other than Binet test returns to Binet interpretations has been deprecated on various grounds, especially with reference to statistical comparability and differential validation. This applies in only somewhat less seriousness to the 'new' Binet (1937 Stanford) and the 'old' Binet (1916 Stanford).

The White House Conference report induced the American Association for the Study of the Feeble-Minded to change its corporate title to that of the American Association on Mental Deficiency, but this verbal change produced no corresponding alteration in interests, programs, or functions. Mental deficiency was considered a more euphonious term with less opprobrious connotations.

Davies' important book (**5**) reflects these same difficulties

coupled with a similar change of title from a former edition. After an excellent discussion of mental deficiency (Chapter I) as feeble-mindedness and as intellectual retardation he presents a history and a program of social control which are almost wholly concerned with the former. This is as inevitable as it is revealing since a program of social control of the socially competent mentally deficient is obviously paradoxical. The feeble-minded reveal a *generalized* subnormality whereas the intellectually retarded reveal a *particularized* deficiency.

An exceptionally comprehensive exposition of these problems in brief compass has been prepared by Pintner (**16**). He cogently reviews the major aspects of feeble-mindedness with probity and balance, employing the term mental deficiency here and there as a synonym. We commend the reader to that summary for a concise elaboration of the present argument.

Pintner evaluates (**16**, p. 808) another concept of feeble-mindedness, the statistical definition advocated by him and Paterson, by Miner and by Burt, as the lowest segment (variously suggested as one-half to three per cent) of the normal curve of distribution considered as a continuum. This concept pays little attention to (*a*) the imperfect correlation among the various test data by means of which the lowest percentiles might be determined, (*b*) disparities between various mental and social criteria, (*c*) the variations with age, sex, milieu (within a given country or century), and other sampling difficulties. The percentage definition by assuming continuous variation denies the symptom-complex nature of mental deficiency, and affords no absolute definition which incorporates the various criteria and attributes of the condition so clearly set forth in Pintner's chapter as a whole. Nor can we agree with his conclusion in this passage (q.v.) that we need two terms, one for mental deficiency as intellectual subnormality and the other as social incompetence. His position is not supported by the studies thus far made by means of the Vineland social scale (**6**). Nor is it consistent with the clinical point of view which requires a mental basis for the social inadequacy rather than a substitute of one for the other.

The IQ concept of mental deficiency is itself a percentage definition. An 'old Binet' IQ of 70 represents approximately the one-percentile (minus 2.3 sigma) and a 'new Binet' IQ of 70 represents approximately the two-percentile (minus 2.0 sigma) of the normal distribution (**15**). The actual percentages and sigmas are not precise due to certain skews in the empirical samples which are somewhat generalized in Merrill's table of IQ classification. Merrill explicitly denies (**15**, p. 648 ff.) an identification of any IQ with feeble-mindedness and clearly asserts the need for clinical determination in a statement of position which accords with our own. It is doubtful, however, if her readers will value this statement more than her table of IQ designations.

Psychologists generally now unwarrantedly accept a 1916 Stanford-Binet IQ of 70 as indicative of mental deficiency, and the term mental deficiency is used interchangeably with feeble-mindedness. This position is explicit and implicit in the most recent *Yearbook of the National Society for the Study of Education* (**22**, *e.g.*, I, p. 50). This is essentially a statistical definition based on Binet IQ alone considered as a continuous variable without regard for multiple clinical criteria. It evades the social criterion which many authorities consider essential, evades the disparity of test performance in battery testing, evades the marked overlapping in mental age for morons and dull-normals.

Such ill-advised simplification of mental diagnosis has been steadily opposed by clinical psychologists and psychiatrists. Research results leading to formulation of social policy, scientific principles, legal practise and educational programs, which are based on erroneous concepts can only do damage to the sciences and the scientists that are party thereto. For if results are published as significant of mental deficiency when the subjects of the investigation are not properly so classified, then these results will be misleading rather than informing. Scientific candor is not so lightly to be disregarded if the social sciences are to command public confidence. The results of investigations bearing on relationships can be no stronger than the weakest term of such comparisons.

The Clinical Concept

The above review only lightly samples the extensive literature on the concept of mental deficiency. We may briefly note that:

(*a*) Mental deficiency has long been considered a synonym for feeble-mindedness and is still widely so employed in law, in science, and in professional practise.

(*b*) In the last three decades the use of Binet MA and IQ has improperly reduced the clinical symptom-complex of mental deficiency to a statistical segment (about minus 2 sigma and below) of the normal curve of distribution of Binet-measured intelligence as a continuum without regard for time and place and other variables.

(*c*) This practise assumes the essential social criterion but substitutes the Binet criterion for it without validation with it, or with other psychological or even intelligence-test criteria, or with the educational sequelae and constitutional origins.

(*d*) Consequently this practise ignores the extensive overlapping in Binet-intelligence between the high-grade feeble-minded and the low-grade normal, and denies the dichotomous rather than continuous variation between them.

(*e*) Mental deficiency thus becomes unnecessarily ambiguous as a scientific term, meaning sometimes clinical feeble-mindedness, sometimes mere intellectual retardation of extreme degree, and sometimes both.

(*f*) This ambiguity leads to regrettable incomparability of data, 'interesting-if-true' conclusions of principle and fact, and dubious policies of legal, social, and educational practise.

(*g*) Scientific rigor requires better logic and more candor in standpoint and evidence to satisfy the demands of plausibility or proof than is evident in published reports of current studies.

Tredgold, the leading text-book authority on mental deficiency (**21**), considers the term synonymous with feeble-mindedness (amentia, oligophrenia, sometimes hypophrenia). He defines amentia (p. 4) as "a state of incomplete mental development of such a kind and degree that the individual is

incapable of adapting himself to the normal environment of his fellows in such a way as to maintain existence independently of supervision, control, or external support." In an earlier (Fourth) edition this definition included "incapable at maturity" and ascribed the mental condition to "a state of restricted potentiality for, or arrest of, cerebral development." These last two considerations are included in his latest treatment although omitted from the definition. His position is essentially that of the Wood Committee (**24**) and of clinical students of feeble-mindedness in this and other countries. Tredgold notes that mental deficiency is a legal and social concept. He observes that it arises from many different causes and assumes many clinical forms. He concludes that "social and scholastic inequality, though often co-existing are not identical." He deprecates the use of the IQ as a single criterion, and emphasizes the wide IQ overlap between high mental deficiency and low normality (Chapter I).

Similarly the Wood Committee (Chapter II) emphasizes that "mere lack of educational attainments cannot, in itself, be regarded as constituting mental deficiency," notes that speaking generally persons with mental ratios below 70 (14-year divisor MA below ten) are probably feeble-minded, with certain exceptions, and concludes that "the only really satisfactory criterion of mental deficiency is the social one."

Without reviewing other authorities we may summarize the clinical concept more specifically as follows:

(*a*) The primary criterion of mental deficiency is one of social inadequacy.

(*b*) This social deficiency is the result of mental deficiency in the more limited sense, *i.e.*, an innate lack of mental capacity sufficient to sustain personal social independence.

(*c*) This lack of mental capacity is due to incomplete mental development during the normal period of genetic maturation.

(*d*) This incomplete mental development is due to constitutional causes affecting the organism as a whole in structure and in function; a generalized hypoplasia, which is most conspicuously reflected in immature cerebral functions.

(*e*) This tendency toward organic, and especially cerebral arrest of development may be the result of natively limited genetic potential (heredity) or of fortuitous post-conceptional alterations of such potential during the normal period of human maturation.

(*f*) The course of development is indeterminate in a particular case, but may be predicted in terms of clinical probabilities according to various circumstances. The condition is therefore both diagnostic and prognostic in the pre-adult period of life.

(*g*) Potential mental deficiency and potential normality are not easily inferred from IQ status at a given stage of maturation (or early life age) because of vagaries of development which are not yet clearly enough known for purposes of stable prediction.

In addition to these general considerations, special attention must be paid to the various types and degrees of mental deficiency. The feeble-minded are classifiable by grade as idiot, imbecile, and moron. They are classifiable by 'form' as primary and secondary, or hereditary and acquired, or endogenous and exogenous. They are also classifiable by type in terms of etiological entities as familial, mongoloid, cretin, microcephalic, birth-injured, and so on. Consequently, feeble-mindedness as a generic category is an aggregate of various clinical syndromes. The results of scientific inquiry vary with these sampling errors.

Psychologists not associated with institutions for the feeble-minded are likely to have but little acquaintance with the more severe degrees of mental deficiency and the relatively infrequent clinical varieties, since these extreme instances do not usually command their attention. There is consequently a tendency to conceive the problem of mental deficiency in terms of the simple (hereditary) moron, since the idiots and imbeciles and the clinical varieties are commonly segregated in institutions or confined to their homes. Moreover, in the extreme cases, diagnosis is simplified by the severity of the deficiency which makes the condition fairly obvious. The extra-institutional psychologist is likely to meet the diag-

nostically borderline cases, namely, those which are encountered at the earlier age levels of genetic maturation or those which are encountered at the high-grade borderzone in the adolescent and pre-adult years.

Among such instances accurate appraisal is embarrassed by the difficulty of determining the social competence of the mentally deficient before such persons have reached the period of adult independence or complete self-sufficiency. The determination of mental deficiency is further complicated by the difficulties encountered in satisfying the several criteria presented above. To escape these difficulties by making the Binet IQ carry the whole burden of proof is as impracticable as it is unsound. By reducing clinical diagnosis to psychometric rule of thumb the demand for 'trained psychologists' becomes insincere. For the psychological values are then inherent in the tests rather than in those who use them. Insofar as such tests are objectively and statistically stereotyped their use and their interpretation require only drill rather than knowledge; skilled clerks or robot technicians would suffice. The very success of psychometric technique thus tends to eliminate the need for psychological insight.

Clinical Determination

The clinical appraisal of mental deficiency requires a fairly comprehensive evaluation of each case in terms of all the significant criteria. This leads to differential diagnosis based on total developmental history and total present status. If this task is difficult, if it calls for wide knowledge and special skills, demands insight and experience, and is withal tedious or time-consuming, we may not merely on that account absolve research students of their responsibility for satisfying the requirements of scientific proof.

Numerous means for establishing such proof are available. They may briefly be summarized in relation to the above criteria as follows:

Social criterion.—The first evidence required is that of social insufficiency. Among adults this is usually estimated from economic and occupational success, personal adequacy

in social relations, prudent management of 'way of life,' and general level of self-sufficiency. Among children and youth during the period of normal dependency inferences as to probable adult social success are drawn from success at school, progress in general social maturation, conduct, interests, companions, and the general 'keeping up with his age.'

These lines of evidence are rather ill-defined and unstandardized. Sophistication and analytic insight are necessary for obtaining an adequate 'impression.' The degrees of success, the influence of related variables, and the conditions of time and place are not easily evaluated. Special abilities and disabilities must be allowed for and environmental circumstances must be taken into account.

But the fact of social competence must be established as a precondition of its interpretation. For this purpose the Vineland Social Maturity Scale affords a systematic standardized method of appraisal. This instrument formulates, defines, and quantifies the social criterion in terms of maturational progression. Its use greatly clarifies this aspect of mental deficiency.

Mental criterion.—The mental criterion serves to explain the social evidence. It differentiates the social incompetence of the feeble-minded from that due to other causes. Degree of intelligence is only one aspect of this explanation since the whole psychology of the individual may be involved. This follows from the marked overlapping in MA and IQ (as measured by the Binet scales) between morons and dull-normals which reduces the differentiating value of such measurements at the borderline. Thus the moron with superficial verbal aptitude may excel the dull-normal with verbal inaptitude since the Binet tests place a premium on linguistic facility which weakens the social significance of the test scores so obtained.

Hence the validity of Binet measures must be supported by well-chosen supplementary tests (non-verbal and manipulative) of intelligence. The interpretation of such battery-test data cannot be reduced to simple rules. The clinician must harmonize these results according to statistical deviation,

inter-correlation, reliability, and validity. He must know the sampling errors, the psychological vicissitudes, the degree of rapport, the freedom from special handicaps, the emotional interferences, and so on which hamper sound deduction. No clerk, no mere technician, but only one 'well-trained' in psychology can do this type of analytic-synthetic evaluation.

But intelligence is only one phase of 'mentality.' What of emotion, volition, temperament, skill, motility, and the rest? When MA and IQ fail to differentiate, the psychologist may resort to the estimation of insight, foresight, resourcefulness, shrewdness, originality, transfer of learning, and other psychological evidences of normality versus deficiency. Recent evidence (**14**) reaffirms that motor retardation accompanies mental retardation among the feeble-minded. They are also subnormal in general alertness (not synonymous with MA), initiative, creative aptitude, sentiments and ideals. They excel in monotonous perseveration and fall short in adaptive concentration. They live by habit, learn by rote and react concretely.

There is a wealth of observation on these non-intellectual aspects of mental deficiency. While it is true that measurement and differential validation of these attributes are in their infancy, these are the directions in which we must seek to substantiate the hypothesis of mental incompetence beyond the point where MA and IQ fail to do so.

Binet adult MA below 8 years or Binet IQ below 50 (assuming valid measurement and no limiting handicaps) will ordinarily be associated with social incompetence indicative of mental deficiency (assuming arrested development), but the majority of the feeble-minded (and all the morons) are beyond these limits and require other means of identification. Here is a major field of further psychological research which is of the utmost significance to psychobiology, and without which we cannot clearly evaluate the related problems of education and social welfare. Psychology itself will profit from such explorations in the light they will shed on the facts and theories of human behavior.

Developmental criterion.—Social incompetence due to men-

tal subnormality must be the result of incomplete maturation (rather than of deterioration from a once-normal adult attainment) to be indicative of feeble-mindedness. This requirement is often ignored in diagnosis because usually the arrested nature of the condition is self-evident. Such retardation may take place either suddenly or gradually at any time prior to normal maturity; it may include cessation or even deterioration during the developmental period from accident or disease without reference to native potential.

The course of maturation is relatively unpredictable, with some familial types 'testing' within normal limits early in life and with other pseudo-feeble-minded cases showing delayed development. Sudden arrest due to fortuitous causes after a genetically determined period of normal development are infrequent and of course wholly unpredictable.

Constancy of IQ cannot be safely assumed for the feeble-minded, and indeed has been authoritatively contradicted. Yet the assumption of IQ constancy continues to confuse students of the nature-nurture problem and is the Nemesis of even those who aim to refute it.

Hence the developmental criterion is more than academically relevant. It points a need for research of practical and theoretical importance. The conditions which favor or impede development and the correlation of the variables involved cannot be clearly evaluated until both the normal and the retarded courses of genetic maturation are more precisely determined.

In clinical practise this evaluation of development is subjectively determined from the history data of the clinical syllabus. This anamnesis includes family history, medical events (prenatal, natal and postnatal), psychogenetic evolution, social maturation, school record, occupational success, conduct, and adjustment bearing on the record of development. There is no need here to give technical details, but the clinician must be versed in such details and standards as will yield sound inferences as to the bearing of these on ultimate attainment.

Here also the Vineland social scale is helpful for certain

purposes in suggesting categories, items, and standards of development which may be significant.

Educational criterion.—The scholastic evidence of mental deficiency is helpful where education is relatively universal, in that it gives important evidence relative to learning capacity and constitutes an indirect measure of linguistic intelligence. Apparent educational inaptitude must be judged with due regard for (*a*) special educational disabilities, (*b*), regularity of attendance, (*c*) adequacy of instruction, (*d*) suitable motivation (freedom from emotional conflict or disinterest), (*e*) verbal handicaps, (*f*) sensory defects, and the like. Apparent educational proficiency may also require discounting because of similarly favorable rather than unfavorable conditions, including particularly (*a*) rote learning, (*b*) drill success, (*c*) superficial verbalism, (*d*) special manual aptitudes, (*e*) docility, (*f*) prolonged attendance or overageness. Educational proficiency or deficiency is only one measure of social competence. Scholastic aptitude, for example, may have a low correlation with occupational aptitudes in given cases.

Somatic criterion.—The evidence on social and mental maturation should be related as far as practicable to anthropometric and medical evidence on constitutional (organic) factors. Here the tendency toward morphological and physiological infantility is reflected in body structure and function. Mental deficiency is theoretically referable to cerebral hypoplasia, including structural and metabolic deficiency or defect. Structural deficiency or defect is not readily evident until postmortem but may be detected by neurological examination in pathological cases. Nutritional, toxic, and metabolic, including endocrine, disorders are specially relevant in some cases.

Evidence on environmental deprivation, emotional interference, sensory handicaps, and volitional attitudes are pertinent. Insofar as these interfere with expressive developmental tendencies at critical moments of maturation the results may, though apparently only rarely, profoundly affect the organism as a whole. More commonly these effects are but temporary and produce only the pseudo-feeble-minded-

ness apparent in cases of delayed development. Environmental stimulation, corrective therapy, and remedial teaching may re-establish normal attainment in such instances without any (other) essential alteration of organic potentiality.

Generally speaking, the clinical determination of mental deficiency is an art which employs scientific procedures. Some departments of the determination have a relatively high degree of scientific validity, others a relatively high degree of subjectivity. This conclusion greatly limits both the facility and accuracy of diagnosis and gives rise to the need for skepticism regarding diagnoses which are too simply determined or which are only wishfully described. Indeed, these very difficulties constitute a major challenge to psychologists in respect to which little progress can be made if the inherent difficulties only intimidate the investigator.

Theories of Mental Deficiency

It should be obvious from the above that no single theory of mental deficiency satisfies all the cases. A consideration of the types and degrees of feeble-mindedness from various points of view leads to several major theories as to the nature of mental deficiency. These theories help to explain the condition and suggest leads for further investigation.

The simplest theory is that mental deficiency represents a simple quantitative deviation from the general norm of human behavior. This theory underlies the use of MA and IQ as the most important if not the sole evidence. It is most plausibly applicable to the endogenous cases in which the total symptom-complex does not reveal a specific clinical or pathological entity. From this point of view mental deficiency, as the very term implies, is merely a want of intellectual power, aptitude, or capacity.

But intimate students of feeble-mindedness are unable to acknowledge that mental deficiency is simply a low degree of normality. Those actively specializing in this field recognize a qualitative variation associated with the quantitative deviation, a unique differentiation from the normal conceived as a continuum. In other words, the *Gestalt* of mental defi-

ciency is different from that of dull-normality. This conclusion follows from the mental age overlapping between morons and dull-normals. The total pattern of the former causes social failure and the total pattern of the latter produces social success. Even the hereditary feeble-minded, numerically the largest group of all the feeble-minded and the cases most commonly seen by the non-institutional psychologist, constitute only an approximation to the theory of a continuous variation from the norm.

In many other cases mental deficiency is a pathological abnormality rather than merely a quantitative deviation. This is apparent from the practicability of determining various clinical entities of mental deficiency as etiological symptom-complexes. Thus the mongolian type of mental deficiency is qualitatively distinct in many respects, physical as well as psychological, from other feeble-minded of the same range of intelligence. Likewise, the other clinical varieties reveal significant differences associated with their pathological morphology and physiological functioning. The very concept of clinical varieties involves a syndrome of attributes which is distinct from one clinical type to another.

Is feeble-mindedness a disease or is it a condition? The answer to this question depends upon the type of case under consideration and the professional approach to the problem. From the point of view of medicine, mental deficiency is a disease, because medical interest attaches to the clinical varieties which represent qualitative variation with basic pathology. But from the psychological point of view mental deficiency is a condition, because of its typically congenital nature and its general non-susceptibility to therapy other than that of educational and social regimen. We may perhaps compromise by saying that in some cases it is a disease and in most cases a condition.

We may conceive of mental deficiency in its morphologic aspects as being essentially ontogenetic, that is to say, a condition which is peculiar to the organism from the point of view of its individual development in relation to a particular species. This theory is a plausible concept of mental deficiency be-

cause of the weight of evidence favoring a definition of feeble-mindedness as a developmental arrest, a generalized individual hypoplasia, a personal tendency toward infantility.

Mental deficiency may also be conceived as an atavism, a reversion to type, a harking back to a primitive stage of social evolution, or a preliterate stage of anthropological progression. This phylogenetic parallelism suggests that some of the clinical varieties of mental deficiency may be compared with earlier forms of social development in man. This thesis is not very closely in accord with the most generally accepted experimental evidence, but does have some value in conceiving of the endogenous types of mental deficiency as reflecting primitive stages of personal-social adequacy. Whereas in the ontogenetic consideration the feeble-minded may be compared with infants in the same social-economic environment, the phylogenetic theory suggests a comparison with the cultural levels of social anthropology. An extension of this thesis is made possible by the comparison of the low-grade feeble-minded with the high-grade anthropoids from which it may be seen that in learning and in adaptive capacity there are significant resemblances.

Implications

The implications of the above postulates are perhaps too self-evident to warrant recapitulation. It is obvious that mental deficiency must be considered not only as a generic term, but also with reference to its sub-degrees and sub-types. The determination of mental deficiency in its entirety involves a clinical determination of symptom-complexes designed to differentiate the feeble-minded from the normal. The further consideration of clinical type involves etiological syndromes as clinical entities for a pertinent segregation of the feeble-minded among themselves.

IQ-determination is distinctly contrary to the clinical consideration of mental deficiency as feeble-mindedness, and is specifically misleading in cases of potential feeble-mindedness and of dull-normals with delayed development.

These implications are clearly apparent in their more

practical scientific bearing on mental deficiency. Research results are of dubious value unless well-grounded on sound differential diagnoses, and may be confused further by the failure to segregate different symptom-complexes within the generic field of mental deficiency. Thus in recent work on environmental stimulation the case for and against nature and nurture must be considered unproved, because of the limited evidence offered on diagnostic status, hereditary background, putative paternity, condition at maturity, and organic integrity. Such studies must not only guarantee the diagnostic status of parent and child but also the etiological nature of such status (if feeble-minded), as well as its degree and its clinical type.

The need for sound diagnosis is further reflected in the gross disparities of estimates regarding the incidence of mental deficiency in criminal and delinquent populations, in schools, in social and racial (or nationality) groups and in cultural areas.

The position then seems sound that psychologists should give much more careful attention to the determination of mental deficiency than is reflected in most psychological work to date. Mental deficiency constitutes a fertile field for research, not only on its inherent problems but specifically with reference to related problems in other branches of psychology. Those working in comparative psychology might well undertake experimentally to produce the animal equivalent of mental deficiency, as has been done in the production of experimental neuroses. Indeed, mental deficiency might well serve as a topic in which the diverse interests of psychologists in many fields might find some happy coordination.

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CONCERNING THE 'FIELD' IN 'FIELD' PSYCHOLOGY

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Organismic biology, which regards man as a system of energy rather than as an atomistic machine steered by the external promptings of an 'engineer,' has made vital progress in the description of behavior in terms of functional (relational) properties which cannot be ascribed to the exclusive action of any one bodily structure or part. These properties of the whole,[1] long acknowledged but long neglected, received primary attention in the psychology of 'Gestalt.' More recently, they have been approached by a related system of constructs which has become known as 'field theory.' It is with these latter constructs that we shall especially deal. Recognizing that not all energy systems are 'machines,' Lewin (**9**) places the organism in a field of 'psychological space' so that its total behavior may be more logically described as arising from the conjoint action of internal forces ('psychic tensions') and external agents ('goal valences'). The mathematical concept of vectors is then utilized, and the locomotion of the organism toward or away from external stimuli becomes the chief indicator of this total behavior.

The similarities between this type of analysis and the

[1] All will agree with Lewin that "it took psychology many steps before it was discovered that a dynamic whole has properties which are different from the properties of their parts or from the sum of their parts. Even relatively recently (in the early Gestalt psychology) the statement was frequently made that 'the whole is more than the sum of its parts.' Today such a formulation can be considered hardly adequate. The whole is not 'more' than the sum of its parts, but it has different properties. The statement should be: 'The whole is different from the sum of its parts.' In other words, there does not exist a superiority of value of the whole. Both the whole and the parts are equally real. On the other hand, the whole has definite properties of its own. This statement has lost all its magic halo and has become a simple fact of science, since it was discovered that this holds also for physical parts and wholes." (*Amer. J. Sociol.*, 1939, **44**, p. 885.)

description of gravitation as a mutual attraction of two physical objects have already been pointed out by field theorists; the differences in the analogy have not always been stated as clearly, however. Thus, a circle can be drawn to define an energy system or field of physical space in which two or more parts with qualitatively similar properties (mass) generate a new total property (gravitational force) by virtue of their quantitative differences. But if 'psychological space' is to have similar meaning, one should not choose to enclose qualitatively different parts (organism and external stimuli) in a common field of operation, with organic locomotion as the emergent property of the whole. Such diagrams are no better and no worse than Loeb's paradigms of 'forced movements' and 'tropistic' responses. This fact, of course, is no argument for an alternative mechanistic 'conditioning' theory; when one must choose between being vague about the properties of behavior wholes and being wrong, 'field' theory in any form is to be preferred.

One can say that it is implicit in the writings of Lewin and other field-theorists that psychological space is entirely within the organism and that inner tensions and outer valences are qualitatively alike in that both are fundamentally neural. Hence, rather than continue this discussion on the naive level of inveighing against the astrological stimulus forces implied in the concept of 'valence,' we will grant to all field-theorists the realization that the properties of behavior wholes arise our of dynamic relationships between neural activities, and that the 'field' of 'field' psychology lies wholly within the organism.[2] Our basic questions concern what such a realization entails in terms of a complete behavioral description and of an adequate experimental methodology.

Granted that macroscopic, molar descriptions of behavior are the aim of organismic psychology, there can be little

[2] Köhler (8) has gone further than most field-theorists in placing all the stresses and strains immediately responsible for behavior in the brain field, and Koffka (7) has built elaborate hypotheses upon the neural trace of past experience. Yet both of these writers, in missing the motor origin of these alleged traces, have lost an opportunity for experimental check of their present unsubstantiated isomorphic doctrines of brain physiology.

criticism of field theory's general point of view. But in getting down to explain concrete cases it is always necessary to go far beyond observed data by postulating neural traces of past behavior operating in the present field. The unsubstantiated character of these traces tends to isolate the field-theorist from the very group of psycho-physiologists who are potentially most able to help him with his dilemma. We wish especially to call attention to a movement, dating from Ribot (**11**) and Feré (**6**), which has been largely ignored or wrongly interpreted. Unlike the many physiologists who are so preoccupied with microscopic observations of parts as to have no interest in, or methods for studying, the problem of how part activities generate behavior totals, these men and their descendants have consistently used physiological measurements of the intact organism, such as galvanic skin resistance, incipient muscular contractions, etc., to *extend* the description of total behavior rather than considering them merely as bodily correlates or substitutes for alleged overt responses and psychological states. This movement, which we may call *dynamotor psychology*, has run an independent course, but so many of its ideas and facts have been fitted into the doctrine of mechanistic behaviorism[3] that the field-theorist has tended to ignore any of its other implications. The writer believes that a more critical evaluation of the work may supply the missing link between the gross behavioral descriptions of field-theorists and their alleged isomorphic brain states, especially by showing how subtle tonic processes reflect and originate neural traces. In the paragraphs which follow we shall indicate briefly the contributions which dynamotor psychology can make to field theory and research.

The point was made some time ago by Washburn (**12**) and ourselves (**2**) that the motor residua of past learning form es-

[3] Only an awareness of the extreme subtleties of response determinants prevents the facts of 'motor' psychology from being adequately assimilated by orthodox behaviorism or conditioning doctrine. One is that a motor act aroused by one stimulus is not discharged by another stimulus through a simple transfer in the sensory part of the reaction arc. Motor responses do more than discharge organic energy patterns originated elsewhere; by virtue of outlasting postural residues and backlash proprioceptive excitation, they actually originate energy patterns which interact in the brain field with excitation aroused by the external stimulus and so cause another response.

sential parts of the field of present perceptual excitation. The structure (meaning) of the total neural action was seen to refer primarily to one part of the field (such as the visual excitation) but to be carried by excitation of other (*i.e.*, motor) origin.[4] The rough qualitative descriptions of behavior which now characterize the field-theoretical approach not only miss these important motor sets almost completely; they often refer such total properties as 'meaning' to the interaction of figure and ground sub-parts within the area of visual excitation. This is especially striking in reversible perspectives when their descriptions in terms of dynamic interplay of 'organism' and 'environment' show only that some parts of the field of visual excitation are more important for one meaning-structure than for another. Such accounts are neither complete nor adequate; at best, they demonstrate only that the meaning-structure is not wholly a property of the visual excitation; at the worst, they ignore the motor aspects of the total neural action pattern which are responsible for shifts in the meaning-structure. They tell that wholes will change after the fact; they do not and cannot predict the part conditions necessary for each new total meaning-structure to emerge. They give a splendid account of *what* we perceive, but their descriptions of *how* we perceive is very incomplete.

It may be argued that all motor activity is more successfully dealt with as results rather than as contributory parts of the total field situation. But one has only to consider the reactions of the experienced rat in the suddenly now-foodless

[4] We do not say that the motor set is the meaning-structure, a view which Gestalt psychologists often try to force upon mechanistic behaviorists and structuralists alike. There is no 'dog response,' as such; 'dogness' is the product of equivalent ways of looking at sets of equivalent objects. But there is no meaning-structure in the absence of these ways of looking, and the more particularized these reaction tendencies, the more differentiated is the meaning-structure. Gestalt psychologists are correct in pointing out the unitary character of earlier experiences in the child but they wrongly refer such unlearned patterns to the mirroring in the brain of already existing and analogous stimulus organizations. The reason the child sees 'global totals' is because of reflex adjustments to visual stimuli, backlash excitation from which interacts with the visual excitation in the brain field. Further perceptual differentiation depends upon additions to this 'core' by learned motor adjustments.

maze to realize how inadequate this approach may become. Here we have forward locomotion resulting supposedly from the dynamic interaction of hunger 'tension' and food-goal 'valence'; but the fact that it makes no difference to the run whether food is actually present or not, whereas any break in movement continuities between starting and stopping box is markedly influential, suggests that the visual image-excitation associated with the food-goal is a result as well as a cause of movement.

Once we get all the energy sources of an animal's present behavior safely located inside its skin, movements which appear to be directed toward distant ends and remote goals seem to be entirely *forced* from within and not pulled from without, after the manner of astrology. We cannot guess toward what these movements are being directed [5] unless we know to what the animal has reacted in the past. Knowing this, we can hypothecate that an overt reaction of past eating has left certain residua which continue into and form a part of the new field of neural excitation. Here the brain excitation of tonic muscular origin (the eating 'set'), identified with and reinforced by excitations from the grosser hunger tension, comes in contact with the visual excitation which represents the start of the maze. The interaction between these two parts of the brain field is sufficiently like that which held between the cortical representa-

[5] The problem of *direction* in overt response presents great difficulties for any psychological system, and it is scarcely to be wondered at that field theory has adopted constructs that make the energies of distant stimulus goals the responsible agents. A less mystical way of looking at the problem of direction involves Bok's principle of the 'reflex circle.' When the organism looks it finds itself seeing something, and, conversely, when it is stimulated by something visual it finds itself looking. But if different visual excitations can develop different ways of looking (or motor excitation) and so create total meaning-structures which appear to be 'out there,' what more natural consequence than that different ways of looking should also create and sustain (subject to occasional reinforcement through retinal channels) visual image excitations associated with those lookings and with the total meaning-structure referring 'out there.' Thus, when the neural correlates of an organic 'need' interact with those of an imagined 'goal,' it is to be expected that the total overt behavior which results from this interaction of parts will discharge along the direction of the 'out there' reference of the imagined goal. The details of how this reaction trace of the past comes to influence the present stimulus field are discussed in the next footnote.

tions of the earlier eating act and the visual excitation connected with the sight of food on that trial that the energy in the entire system is released as a new overt running movement directed toward the food end of the maze.[6]

This forced locomotion, a property of the dynamic interaction of part processes in the brain field, by discharging the organic energies or tensions, in turn initiates a new tension system which begins building up for later discharge.[7]

When the motor discharge of a field of neural energy representing 'needs' and 'goals' is blocked by interruptions or thwarts of various types, the original energy system continues to build up its excitation level by motor reinforcement until it breaks through the inhibiting excitations to some form of overt response. It is at this point that we must part company with orthodox conditioning doctrine; for, while many instances can be found where behavior directed toward remote goals is adequately explained as being set off by some specific aspect of the present stimulus situation to which that behavior was earlier associated, there are times when the urge or inner tension basic to goal-directed behavior attains such intensity that it forces discharge in almost any stimulus field. The dog whose sex tension has reached an optimal point will, in the absence of mate, run around and around and

[6] From this point of view we see that field theory has been leaving out considerable relevant data by the neglect of present 'set' conditions which originated in reactions of the past. We must deny the notion of psychic continuity between the perception of the start of the maze and the image of the food at its end, and deal only with movement residua. As I look at the photograph on the wall in front of my desk, then turn and face a visitor entering from an opposite direction, my only present notion that the picture recently viewed is in the same room as my visitor is that I have made similar looking movements to both. The movement residua of past looking, carrying over into present looking, unite the present perception with the past. Thus, I can impress the referend of past looking (the photograph) upon the referend of present looking (the visitor) and, noting a certain dynamic equivalence, can discharge overt response through the present object toward the past object, as I say to the visitor, "You remind me of a photograph in that direction beyond you." In the case of the rat, it is the equivalence of running movements made at the start and end of the maze which permits the energy system, built up by interacting hunger tension and 'start' excitation, to discharge through the 'start' stimulus toward the 'end' stimulus.

[7] An extreme instance of this principle is found in the compulsions of psychotics, though to a lesser extent it holds for all behavior. The moment one begins to smoke a cigarette, he is starting to charge his system for smoking again. Instances of this principle have also been noted in conditioning experiments (1, 5, 10).

finally rub himself and discharge on a tree or post. Such examples of *functional autonomy* make one suspect that the external stimuli toward which we have made movements in the past do not actually initiate movement sequences *de novo*, but only interact with weak traces of former movements already operative to lower their thresholds to a point where overt discharge can occur. If the traces reach sufficient strength (by reinforcement from basic tension or charge), a less adequate, or even a previously inexperienced, stimulus excitation may interact with them to cause overt responses; conversely, if a specific stimulus situation is long connected by habit with movements which discharge some basic tension, this stimulus can bring the traces of former movements to overt threshold strength in the relative absence of the basic reinforcing tension.[8]

And even if this stimulus is displaced, as by interruption, the motor activity which has been initiated tends to carry through to eventual overt discharge. The continuation of interrupted work requires no special concept of 'psychic' tension for explanation; behavior may be more adequately conceived as a function of quantifiable motor conditions and the algebraic summation of their excitatory and inhibitory volleys (3).

The point has been made here and elsewhere that the only continuities in field psychology are continuities of movement residua and not of epiphenomenal exteroceptive images or sensations of 'means' and 'ends.' Each overt reaction, a product of persisting proprioceptive excitation and of newly introduced exteroceptive excitation, leaves its own postural

[8] A crucial experimental problem here presents itself. Could not an animal be so trained by proper satisfaction of one basic tension (mate) and deprivation of another (food) in the same maze that, when faced with a choice of goals in that maze, he would run to the mate although relatively more satiated with it than the food? Furthermore, could not his choice be shifted in predictable ways by varying the amount of satisfaction and the specific training in ways of realization for each need, with need optimal for one and training minimal for the other behavioral pattern? The results of such choice would throw light on the primacy of habitual response over new untrained need. This question is possibly related to the 'substitute' experiments, now being conducted by the Lewin group, where the more habitual the specific approach to a given goal the greater the resistance to substitute behavior.

residua. This motor condition, unless shifted by reinforcement or inhibition from other sources, runs a predictable course of decline until the pattern reaches an excitation level no longer observable (by action potentials) at the periphery, but remains only as a weak gradient of altered potential in the central nervous system to influence response to new stimulation.[9]

We return now to the problem of the adequacy of present field theory. As long as we observe only the rapid, gross movements of overt reaction (as when the more enduring covert 'set' breaks out), we may be fairly content to conceive total behavioral description in terms of an 'organism-environment' interaction—partly push and partly pull. As soon as we begin to study tonic residua of the overt phasic response, however, we find that we have added the necessary link or part of our dynamic field which will sustain and force new overt reaction to new phasic excitation. By such inclusion we have established continuity between the present and the past.

We have also added commensurably to experimental methodology, for now we will measure tonic as well as phasic reaction processes, and quantify the amount of tonic (covert) arousal and the amount of phasic (overt) expression by suitable instrumental procedures. These are methods which we and others have been using with considerable fruitfulness for over ten years. Our studies indicate that tonic muscular processes are not simply another class of overt locomotor reactions; their primary function apparently is to place a tonic excitation in the brain field, on which new phasic excitation focalizes.[10] Total behavior, in the sense of overt (phasic) response or locomotion, is a product of this interaction. The 'field' of 'field' psychology is within the organism, and the essential

[9] The experimental and theoretical exposition of these motor residuals of past performance to motor 'sets' of present performance is the subject of several papers now in press or preparation.

[10] While we have discussed in this paper only the sustaining, reinforcing, and inhibiting effects of tonic processes of skeletal muscle origin, elsewhere (4) we have recognized the contributions which smooth muscles and glands make to the tonic excitation background.

parts of that field are tonic-interoceptive and phasic-exteroceptive excitation patterns. Since these part patterns, qualitatively alike, differ in time, intensity, spread, locus, etc., we can expect them to generate all the new properties of behavior wholes by their quantitative interactions.

It is, perhaps, too early to urge this detailed, quantifiable attack upon field-theorists to the exclusion of their present qualitative descriptions at the superficial, organism-environment level.[11] In many areas, as with social phenomena, such rough qualitative constructs will serve a useful function for some time to come. In the more finite areas of individual psychology, the contention that our tonic-phasic descriptions offer a better basis for the explanation of total behavior must rest finally with the verdict of another decade as to which method of attack has been more productive in the development and organization of facts.

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[11] The writer is at one with the Lewin group in the belief that 'systematic' rather than 'historical' analyses should be applied to the problems of psychology; but the temporal factor cannot be totally neglected—for, as we have shown, short-lived motor residua are part of the present field. This approach is to be viewed not in contrast with, but as an extension of, present field theory.

MENTAL MEASUREMENT AND COLLEGE OBJECTIVE[1]

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Among various groups of insects, particularly the hymenoptera, there are commonly recognized three stages of metamorphosis, each with a dominant function. The larval stage is characterized by food-getting; in early Jungian terms, an alimentary fixation of libido. In the phase of pupation, development of a structural nature is emphasized. The final, imago stage is winged, and is more especially concerned with reproduction.

The compiler of a modern bestiary could find many analogues between these processes, and the development of the matured scholar in psychology. During the initial phase we observe a dominant investigative drive, a hunger after new data, the end products of whose assimilation are more or less continuously excreted in the form of printers' ink. Upon this phase there normally supervenes a period of intellectual pupation, during which the body of observation and reflection becomes structuralized, concepts are formed, and a psychological system is developed. When one is so fortunate as to accomplish this process he becomes the matured scholar, founds a school, rears intellectual offspring, and his feet get further and further from the ground. Now there are certain kinds of data whose ingestion in a larval phase acts inhibitorily upon later development, and these are specially abundant in the applied fields of our discipline. Those feeding on them often become fixated on the clinical and the particular, so that their conceptualizing conceives only with great difficulty, and almost never reaches the fruition of the school. The heartiest acknowledgement is due therefore of your

[1] Address of the retiring President, Eastern Psychological Association, Atlantic City, April 5, 1940.

graciousness in here bestowing the honor of your presidency upon a grubbing larva in the consultative furrow, rather than some high-flying imago of the factorial treetops. Nor do there seem any two functions touching the scientific interests of so many of us as those of advanced education, and the quantitation of the mental processes there involved. The effort here will be to discuss some ways in which they impinge on each other, and on some wider problems of social organization.

It must be understood that the American college is a quite specialized institution, whose counterpart has developed in no other educational system. The pattern seems to have arisen to meet a new society's acutely felt need for institutionalized learning relatively advanced (much emphasis on the relatively, according to Keppel), while of traditional content. Its academic functions were to diffuse knowledge rather than to extend it. As the culture advanced, and need for the latter function asserted itself, there was superposed upon the college, a borrowing from the university system of continental Europe. Superficially the ensuing system appears cumbrous and heavy-footed compared to the traditional German model of Volksschule, gymnasium and university.

But a far-reaching difference in educational function is involved. There the traditional, and lately far more emphasized function of education is to induct the individual into the service of the state. Here it has been to further one's self-realization as an individual. It is largely on these grounds that the intrusion of the college between secondary and university education would justify itself in a libertarian culture. At least theoretically so; let us now attempt some examination of what functions American colleges are actually supposed to perform, and how far they do what a culture like ours has a right to expect.

The least debatable function of the college is preparation for research and the higher learning. The most specific question here is its value in comparison with direct transition to the university, from a secondary school system reaching to approximately the level of our junior college. It is believed

there is a good case for this lengthening of pre-university work.

A second function of the American college, and if we should believe Columbia's Professor Edman (**5**), the principal one, is as educational Joseph's coat. The genial originator of leisure class theory scarcely gave due credit to this archetype of conspicuous consumption. What Jacob did was as though the father of David Harum or of Al Joad should present the favored son with an outfit of sport clothes from Abercrombie and Fitch.

That is to say, the garment derived its honorific character through denoting by its unserviceability an exemption from workaday effort. The principle inheres in clothes of ceremony generally. Veblen's (**24**) contribution was to develop the principle into a fundamental economic thesis. The less economical the more reputable, as denoting superfluity of resource. Briefly to illustrate some of its ramifications. It is less certain and effective, but much more reputable, to take trout on a fly than with a worm. The pothunter, who kills for food, has less status than the sportsman, who kills for recreation. Longer memories may recall a disrepute formerly attaching to what is now termed 'beauty culture,' because such practices implied that the qualities sought were to be made pecuniarily useful, rather than serving as tokens of decency in a leisure class. The conventions of personal giving are among the best cases in point. To give something in the class of necessaries carries the derogatory implication that the recipient could not supply it from his own resources. The luxury gift carries the assumption that needs are amply supplied. Generally negative honorific value thus attaches to gifts of cash; hence the double meaning of the phrase, to take the cash and let the credit go. It is similar with staple foods, while luxury consumables are reputable; a box of cigars or sweetmeats may be offered where a sack of potatoes would be an affront. In this way derives the special gift status attaching to flowers. In the case of articles manufactured for gifts, utilitarian functions are especially subordinated to distinctiveness of material and novelty of form. Contrast the standard library

bookend with the gift instruments of this function. Hence the pointlessness of such movements as the Spugs so-called (society for the prevention of useless giving); since in its inutility, the honorific essence of the gift resides.

To this function the liberal arts curriculum readily lends itself, by reason of its essential, often explicit, detachment from utilitarian functions. Liberal arts education becomes an honorific pursuit, a sort of vicarious consumption, attesting the socio-economic valence of the family. Hence arises the term 'rich man's college,' meaning an institution which has acquired special repute, such that attendance there carries a *noblesse oblige* to conspicuous expenditure. A reasonable implication is reflected in this utterance by a teacher in one of our high-standing autonomous colleges, in the local student paper:

> A college is no better than the men it has to work with, and it learns to be thankful for the few intellectuals that come its way . . . learns to tolerate the man who uses the college as a practise exercise in how to sell himself to the world; and tries . . . to learn how to be patient with those who do not like intellectual discipline and who under no circumstances will ever learn to like it. These are the 'compact majority' that make up the bulk of the college. To these the obligation of . . . is to maintain a decent standard of work, and to catch as early as possible those who can't or won't attain it. This is much more difficult than is sometimes realized; but to accomplish it is the ignoble task of the college.

In lighter hearted vein one meets with merry quips such as raising the dough for a four-year loaf, or that it is better to have gone and loafed than never to have gone at all. These things are said of liberal arts colleges; and according to leisure class theory, we should expect of them as such, a higher honorific status than of colleges with relatively vocational functions. In this country it is not altogether the case; it cannot be examined without invidious comparisons, but I doubt if there is particularly more repute in graduating from Stanford or Yale than from Caltech or MIT. Our newer

culture has not, and may never evolve so far on leisure class lines as those of England or pre-war Germany, so that utility retains with us a disproportionate share of repute. But in Germany the liberal *Universität* coordinated with the *Technische Hochschule*—the names carry their own connotations; and where are the English technical schools with the honorific status of Cambridge and Oxford? In this country the principle seems illustrated in the two colleges located in a town near the geographical center of my own State, and to apply more generally in women's colleges, as for example, between Bryn Mawr, Vassar, Wellesley and Simmons. Sometimes it comes out as well in public secondary schools, in social attitudes of pupils following the liberal arts or 'classical' curricula towards those constrained to follow courses in the useful arts. Compare, again at more advanced levels, the honorific differential between the terms 'research' and 'service.'

An operation of this principle may be traced in the relative academic accomplishment of students in the same college who have come from public or private secondary schools. The latter include, according to Sargent (**18**), about one per cent of the country's school population, and may be looked on as highly selected for socio-economic status. According to most psychometric comparisons, this is to some extent also a selection for intelligence test status. It is the general experience at my own institution and apparently elsewhere, that students coming from public schools do better academically than those from private schools; that is to say, the lower pecuniary status goes with the higher academic performance. In any case some leisure class function of college education appears; the socio-economically superior furnish either the less able or the less assiduous. Informed opinion seems to favor the latter; that intellects of the private school group are at least as capable, but are the more diverted to leisure class pursuits extrinsic to the academic life.

A curious sidelight on this matter, a pendant to some work of Dudycha's (**3**), comes from the appointment records of a study on which more later. About 60 students were concerned, and with the various members of the research staff, an average

of some 15 appointments was made and kept for each student. The present concern is with those appointments made which the student broke without notice, simply did not turn up. Certain casual observations suggested the plotting of this tendency against the private allowances of the students. The group comprised 18 cases in whom this allowance was under $1200, 41 cases in whom it was $1200 and over, ranging up to $4000. The median per cent of appointments so broken is for the low allowance group zero, for the high allowance group 9. Fourteen per cent or more appointments were broken without notice by 3, about 17 per cent of the low allowance group; and by 12 of the high allowance group. The highest of these percentages is 54, the next, 40. Both students concerned are most amiable young fellows; in no case can these reactions be ascribed to an attitude of conscious unwillingness, nor is there any reason to suppose that less casual attitudes would be expected of others, than are extended.

The disjunction between capacity for higher education and opportunity to receive it has lately been reaffirmed by Goodwin Watson (**25**), by A. M. Jordan (**9**), and by Toops (**23**). The extent to which economic factors enter into such a picture is reflected in some studies made in connection with an NYA project in Boston. Alpha test scores were available for some 1200 NYA cases, whose families were of relief status, slightly less than 4000 cases of normal YMCA population, as well as of course the army standards as recalculated by Bregman (**1**). Bregman's point score median for the army group is a little over 60; that of the YMCA group approximates 130, that of the NYA group 110. Considering alpha 150 as a fair college student median, this figure is surpassed by 4 per cent of Bregman's group, 26 per cent of the YMCA group, 11 per cent of the NYA group. It hardly makes sense that a group of relief status should be in alpha score superior to the population at large, and the disparity must be referred to the manner in which this NYA group was selected, but there is indicated a considerable extent of educational frustration on grounds essentially economic.

Accordingly, besides being the gateway to higher learning,

the liberal arts complex is among our culture's most reputed agencies of conspicuous consumption. Against this 'profitable prolongation of the period of infancy' as Keppel (**10**) has termed it, can be urged the exclusion of students pecuniarily less qualified but intellectually better equipped. For our cultural setup it may be a valid reply that through serving in this way, higher education has acquired material resources it would otherwise have foregone, through which resources it has rather accomplished a greater diffusion of its influence through all economic levels. Or it can be urged, as Link (**14**) has done, that the four-year loaf does not give proper sustenance for later and closer struggles with reality. Neither is this argument easy to appraise, and the case method only is relevant. It is patent that one could loaf his way through college so far as academics are concerned, while acquiring concurrent experience of not incomparable educative value. But in so doing one is apt to interfere with others who are in college for more recognized purposes. While conspicuous consumption under the aegis of formal education is by no means peculiar to the United States, it is perhaps the aspect most detached from the college's traditional functions, as well as debatable from the standpoint of social value.

A third, probably most typical and real function of the American college is to provide terminal education for a fairly wide upper intellectual stratum. This is a task more complex than induction to higher learning or the professions, or keeping a coat of many colors cleaned and pressed. The former is less difficult to validate, the latter scarcely needs validation. In the present context the college is viewed as a school of the organism as a whole, and it is to be assessed according to total and subsequent adjustment of the individual. No practical way has been found to make a full assessment, though there are ways whose validity is serviceable if partial, such as Who's Who status. It is important to keep in mind that college populations are already selected, and to appraise closely the influence of the college one needs to study similarly selected individuals who do not go to college. The method of co-twin control, once suggested by a colleague in physiology for the

validation of certain religious practices, is little applicable. A shred of evidence is offered by a previously reported study on high and low earning business men, where in the presence of similar cultural backgrounds, there was no apparent influence of college on the economic future; but such a property is scarcely claimed for liberal arts training anyway. Perhaps the most competent consensus of opinion from this angle is represented in the *Fortune* poll of February 1939. As may be recalled, there was no pronounced trend; that the college man is delayed in starting is obvious, some believe the handicap is rather more than made up; but how far this is due to college influence, and how far to the manner in which the college population is selected, remains an open question. It is easily understood that the college was favored by professional people, the reaction of business executives was predominantly negative. That college prestige showed to best advantage in this section of the country and much less well on the Pacific coast is probably a function of tradition.

Comparisons somewhat more satisfactory, if also more invidious, can be made between different colleges, but the selectional factor again returns to plague the inquirer. If one compared graduates of Dartmouth and Columbia, one might well find significant differences in their career types; but how far is this due to what Columbia or Dartmouth have done to them, and how far to different types of men who select Columbia or Dartmouth? This is a complicating factor in otherwise very useful comparisons such as made by Prentice and Kunkel on the basis of Who's Who representation. Places like Yale, Princeton and Harvard have been among those having more than their share of Who's Who men, but as Willoughby (**26**) has pointed out, we cannot say how far this results from experiences they give, or because their prestige attracts persons with mental endowments and/or social status favorable to Who's Who achievement. With reference to matters of general educational policy, it may be noted that in the Prentice-Kunkel (**11, 12**) data for both 1928 and 1939 autonomous colleges rank well among those of the great universities.

Other criteria are on the whole less satisfactorily developed. A study made some years back reported a nearly 3 : 1 disproportion of college men in certain penal institutions, but the sample was scarcely suited to breaking down according to colleges involved. Though pertinent enough, it would be most difficult to compare a series of institutions according to graduates having relief status, though a few such data appear in the recent salary study by Greenleaf (**8**). In the course of this work, there is also a salary comparison for institutions of over and under 3000 students. Among the men, the so-called small college salary is for the first year (classes of 1935) 95 per cent of the large college salary, with an average difference of $61; for the earlier extreme (classes of 1928) the small college salary is 88 per cent of the large, with an average difference of $284. For the women graduates there is no evidence of such a progressive disparity, but the disparity itself is greater; the small college salary runs only some 85 per cent of the large, representing a difference around $200.

Scrutinizing the colleges actually concerned, it seems unlikely that curricular differences between the two groups are effective here. It is quite conceivable on the other hand that the numerically large institutions are in general better situated for alumni placement, and would have somewhat more of the best earners than would the smaller institutions.

It is desired to emphasize that the effectiveness with which a college performs its task depends not only on intrinsic merits of its teaching staff and policies, but on the manner in which the student group is adjusted to these factors. To those who take at least individual differences for granted, Fink and Cantril (**6**) have done a notable service in directing attention to qualitative, non-invidious differences subsisting between college institutions. Something of the sort has long been attempted with private secondary schools, but the differentiation of colleges is not less important educationally, and ought to be easier. What is in mind now is the problem for the student, if not of making the best choice at least of avoiding a wrong one. It is recognized that for the more reputed institutions it is just now a seller's market, these colleges can

pick and choose and they do; but is it well that the intelligence in this choice should be so largely on one side? A college may properly elect to gamble on some proportion of its admissions; how far those persons who act in the student's interest are similarly justified, is another matter. Certainly he and they should understand the risks taken, for example, by anyone who goes to a first rate university college with the intellectual equipment represented by an SAT of less than 450. Not that the risk is illegitimate, but it should be taken with eyes open by both sides. The questions, is this boy a proper bet for Columbia College, and is Columbia College a proper bet for this boy, are not wholly synonymous.

The search is for some process to improve selection in the interests of both college and student, but with more emphasis on the latter than has been given. Even assuming this to be a part of his functions, it is not practicable for an admitting officer under the existing setup to assemble the information necessary to the wisest decision in the applicant's interest. The colleges should be able to use more information than can be got from school records, group tests, *ex parte* testimonials and perhaps a personal interview. The student, if he does not start from zero, may have his choice actually handicapped by considerations of prestige, tradition, and information from biased sources.

These considerations have raised with perhaps many of us the question whether there should not be a more organized process, devoted to the wiser planning of a student's post-secondary education. A most promising beginning is actually to be made this summer, under the leadership of Leonard Carmichael. My own first idea was that a freshman college year could so serve, but very serious difficulties have been pointed out. It seems well worth considering what might be done with a period of say two or three weeks, during the vacation period preceding the final secondary school year, at which time groups of intending college students should be assembled for such observation as would assist in the elucidation of these problems. Such an institute could use an existing educational plant, and could well be under the supervision of personnel

officers from colleges interested. During such a period, detailed study of aptitudes and interests could be made, and far more valid data on personalities obtained than is possible by any means now available. Well administered, such an institute should be more than self-supporting, for anyone who can afford to send a son to 'college'—as such a very unsophisticated proceeding—could better afford the fraction of cost involved in securing that the college program was organized in a well-considered way. The conduct of such an institute makes as exacting demands on professional honor as on professional knowledge—may one hope that this, at least, is not among the obstacles.

The frequent inclusion of college and university functions under a single institution raises many questions concerning their relationships. In part the answers must depend on the roles played in the undergraduate departments by the above three basic collegiate functions; (*a*) induction to higher learning and profession, (*b*) conspicuous consumption, (*c*) school of the organism as a whole. It is impracticable within the frame of these remarks to compare some notable conceptions of these college functions as set forth by various distinguished educationists. A challenging question involved, of much concern to the institution I represent, is the relation of teaching to research. An attempt to review this matter objectively involves such topics as the varied meanings of research; student attitudes towards instruction in relation to the research interests of teachers; after-careers of students from colleges with differing emphases on research.

In any case, the relevant criticism of educational philosophies consists not in attempts to find one true at the expense of others, but in understanding the ways in which each came about, and the different senses in which each is valid. It is being increasingly recognized how viewpoints and life philosophies of all kinds are functions of the endowments and experiences of their holders, so that philosophies of education as well as statecraft must be interpreted in considerable part through differentials in backgrounds, triumphs, and frustrations of individual psychogenesis. Objective foundations in educa-

tional policy are not to be determined without student case study, and as an educational institution adopts a distinctive program, it is fair to take an interest in how that program works out in terms of those who pass through it.

This is in some part the function of a research project set up at Harvard University during the past two years under the name of the Grant Study in Social Adjustments. The basic aim of this work is to learn about any distinctive characteristics of individuals making superior and socially benign adjustments to their environments. Natural and convenient, though in no sense the only, source where such individuals might be found, are institutions of higher education. It can hardly be questioned that a college population represents in general a superior and still not too specialized stratum of the society as a whole. It is equally certain that not less superior individuals exist outside of college; but if one wished to ascertain the characteristics of a superior group of young American adults, what gave them superiority, its objective manifestations, how it individually develops, good reason could be cited for looking to the college for one's material. You would be justified in expecting at some time in the work the study of non-college groups. We should not repeat on a larger scale the sentimentalism of the educator who dedicated his book to Theodore Roosevelt who having "wrought in the world what he was taught in college (has) shown the power for good a college man can be." There are too many negative controls.

The chief concern here must be with the mental measurement implications of the work, but it is well to make clear the setting in which these are being derived. The research unit now consists of an internist who also functions executively, two psychiatrists, a physiologist who is also trained in psychiatry, an anthropologist, a social investigator and a psychologist who functions in the field of measurement. The 1938 series of observations involved some 55 to 60 individuals, referred by the Dean's office as satisfactory students; during the present college year a larger number are being studied, with a selectional policy modified in the light of the 1938

experience. The amount of time that each student gives, averaged for the 1938 group about 18 hours, of which rather more than half were devoted to psychiatric data, and a little over two hours to psychometric work.

The selectional policies utilized for the 1938 series resulted in a group of students disproportionately high in economic status, as has already appeared; but they apparently do give, in most psychometric respects, a serviceably close picture of the trait-distributions for the college as a whole; at least for those with satisfactory records.

Some of the more familiar procedures yielded findings such as that Alpha scores had a median of 163, and ranged from 198 to 106. Kuhlmann-Binet opposites time, a very simple speed function, had a median at 27 seconds, with a range from the excellent speed of 17 to something over 40. Otis arithmetical reasoning scores ranged from not over the rather modest score of 10 to maximal. Multiple choice vocabulary lists of the 1916 Stanford-Binet type scored to a median percentage of 86, ranging from 71 to 97.

The immediate problem of personality organization is approached from three main directions, morphological, physiological and psychological. Comparisons among these have usually taken the form of search for correlations among two variables, as between pyknic and extravert, or leptosomic and schizoid. These comparisons have had just the degree of success not to discourage further study, while giving evidence that the categories are over-simplified and not always well-conceived. This is the apparent meaning of the many indubitable disharmonies that are observed. One might then ask, for example, if there is consistency between morphological, physiological and psychological findings denoting a schizothymic reaction type, is this personality likely to be better organized as schizothymic than if morphological and psychological factors were schizothymic, but physiological ones cyclothymic? Obviously no simple answer should be looked for, but the present setup offers unusual opportunities for dealing with it, and one facet of the problem may be described.

From the anthropometric standpoint a number of morphological types have been given racial designations. For each of these types is postulated a line of historical development, so that one may state whether a given individual of say Nordic-Alpine type is morphologically advanced along the line of modernization, or represents a relatively conservative somatotype for this so-called race. Anthropologically, the former situation is termed flexible, the latter rigid. Questions may be raised regarding psychological correlates; whether, broadly speaking, a flexible somatotype of whatever race may have the more modifiable or plastic mental attitudes, and *vice versa*. Among psychometric techniques are certain ones giving scope for patterns of flexibility and rigidity in the above senses. For example, a Rorschach correlate of rigid somatotype would be a pattern restricted to F+'s, while a flexible one might embody FC's and Vistas. When comparison was made of independent somatic and psychological classifications for the 50 odd cases concerned, there was full agreement in 18 cases, full disagreement in 8, the remainder indeterminate, usually on the psychometric rather than the anthropometric side. All but one of the 8 full disagreements were psychologically flexible against rigid somatotype. There is no evidence that the personalities showing this disharmony are less well organized than the others; indeed this class contains some of the best organized in the series, one of already very exceptional achievement and promise. From such data it would appear that while somatotype may give a bias to behavior pattern, this is by no means determining, and may be heavily overlaid by psychogenic factors, at least in the direction of flexibility. That flexible and rigid as here used are provisional and oversimplified categories should be conceded without question. For example, they do not distinguish between the flexibility of annealed rods and that of tempered springs, between plasticity and resilience, both of which have their human counterparts.

Nor should the impression be given that the group is committed, or even inclines, to psychogenic rather than constitutional interpretations of personality. They have been much

impressed by features in the material like those brought forward by Thom (**21**), in his studies of superior adjustments under surpassingly difficult psychogenesis. It is even probable that the present observations have led to a more constitutional bias than is usual among groups with interests predominantly psychological or educational. The issue is not discussed here, beyond raising objection to the heuristic argument for environmentalism. Previous reference has been made to the resulting use of heredity as a scapegoat. When one contemplates the sort of environment the species creates for itself, one could better be grateful for the outfit of age-tried chromosomes by which one supports this environment.

For the 1938 series there were adopted from a psychiatric standpoint certain classifications, among which it was convenient to include the battered clichés of introvert and extravert. In relating these classifications to the psychometric data, it is pertinent to note that (1) the present writer had negligible part in formulating these categories or assigning the classifications, (2) the information which enters into assigning them is extensive and intimate to an unusual degree. These and other classifications should be substantially as valid as the categories themselves.

In respect to the conventional definitions, the ordinary word-association test did not show either in response-commonality or content relationship, the relation towards the introvert-extravert categories that is called for in theory. Such relationship did however appear with a category denoted 'judgment in face of problems'; and in a psychiatric classification of normal and not-normal, the normal classification predominated heavily where the subjective responses were few.

More promising leads concerning introvert-extravert categories are opened up by Levy's observations with overprotected children. To quote his words,

> When an over-protected child goes into the first grade, he has a great advantage in reading, at least a great advantage in verbalization. He succeeds easily, therefore, in subjects requiring reading facility and verbalizing

> ability. However, when he strikes a subject like arithmetic he has no advantage, in fact in some cases less, because he is often free of usual responsibilities, *e.g.* going to the store on errands and the like, escaping thereby a number of experiences requiring number concepts. Generally, in response to a difficult subject, in line with his general response to overprotection, he takes very badly to a subject in which he is not favored, increases his activity along the lines of his facility, and thereby the discrepancy between the reading subjects and the mathematical subjects becomes greater (13, p. 31).

There are essential similarities between this view and one put forward somewhere by Alfred Adler, that the mathematically minded person finds his sources of mental security within himself, the verbally minded one seeks them rather through inter-personal relationships. The truth common to these formulations has been obscured by the invidious distinctions that have grown up around the introvert-extravert dichotomy. It is better to take as a starting point the comparison of verbal and mathematical abilities. For present purposes an available, if not quite flawless representation of this is found in the comparison of the standard scholastic aptitude and mathematical attainments tests. The chief difficulty with the comparison is implicit in the designations of aptitude versus attainment, and it is likely that a series confined to arithmetical processes would be better for this particular purpose. The difference between the two functions does, however, furnish some criterion of the degree of verbalism. A difference of as much as 100 points of raw score is well marked, though I have seen it nearly 400. In nine cases of the 1938 Grant Study group, there was a deviation in either direction amounting to some 100 points or more. When these individuals are checked with their psychiatric ratings, it appears that mathematical aptitude is a special asset if one is to be an introvert and like it. Implicit in this are the following: Relative verbal ability is normal for a well adjusted extravert. Relative mathematical ability is normal for a well adjusted introvert. An introvert relativeiy verbalized is less

likely to be well adjusted, and conversely for the extravert. There is some casuistic material to indicate that these unadjusted introverts and extraverts do not basically belong to such classifications, but have developed these patterns in response to special environmental stresses. The simpler and probably more frequent case of this kind is an originally outgoing but oversensitive personality which retires on itself in the face of wholly normal socio-economic frustrations. Less frequent, but observed, are cases where the problem appears to be a basically introverted psychology harassed by social pressures into an extraversive masquerade.

The psychometric aspect of three cases may be briefly cited in illustration. First a pattern of verbalist-extravert-adjusted-normal. Entrance SAT and MAT had differed by some 200 points in favor of SAT, which was well into the 600's. Alpha score is in the upper 180's, closely fitting the regression of this group's alpha upon SAT. Internally, a relative weakness of the analogies function compared with the opposites is noticed. Easy opposites were fastest but one in the entire group, vocabulary range among the first dozen, verbal productivity in an essay type of procedure somewhat above average in quantity and among the highest in rated quality. The MAT status already noted is reflected in a performance only average in alpha subtest 2, below average in simple computation, nearly the lowest of the group in Otis Reasoning. Allied to these processes may be the relatively low performances in syllogistic reasoning, and markedly low scoring manipulative functions. The word association response is clearly subjective by both criteria, contrary to theory.

This is the psychometric pattern of a very successful student, with balanced activities on the extracurricular side, with external circumstances moderate but not straitened. He is among those rated as flexible in both somatic and mental measurement.

Critical similarities and differences appear in the relation of this case to the following, in which the verbalist pattern is associated with classifications of introvert, unadjusted, with

questionable 'normality.' Like the previous case, he is among the marked excesses of SAT over MAT, though not so great as the other. In alpha score he is somewhat the superior, and again fits the SAT regression line. Vocabulary range is distinctly the greater, in fact among the highest observed. In the selective functions generally, there is a level of accomplishment approximating the upper .1 of one per cent. Number functions on the other hand are not more than average for this group. Together with the manipulative functions they are somewhat better than in the previous case, but still greatly below the selective verbal functions.

It is in the creative or autogenous type of verbal responses that the two cases really part company. In spite of selective functions clearly higher in the present case, the creative ones are notably lower. In such findings, psychogenic inhibitions are always to be considered, and from the standpoint of the history, are present to a considerable degree. Anthropometric and psychometric data again agree, now in that conflicting dispositions are present; and the physiological pattern is given as one of instability.

In spite of these constitutional factors, the history with its collateral data leads one to look on the adjustment difficulties as having a large psychogenic determinant. The pattern is consistent with, and suggests, an introversive adjustment on the basis of external frustrations.

The third case, on the other hand, shows a pattern of marked excess in entrance MAT over SAT, here rated as introvert, normal and well adjusted. Consistently, the psychometric findings include an alpha score somewhat below the local average, through the verbal over-weighting of this series. In contrast to the first case, the opposites items are notably low, the analogies high. The arithmetical work generally is among the highest in the group, with syllogistic items also above mean. Manipulative functions are quite good; the configuration could be a textbook case for Freyd's (7) observations on the 'mechanically inclined.' Creative verbal response is relatively inhibited; in word association response the pattern is subjective, which is theoretically typical for

the introvert, but which does not so characterize this material as a whole.

In general personality organization this man has strong and competent interests in dealing with things. While adequate to serve these interests, ideational and social capacities are subordinate to them. Somatically he rated on the rigid side, psychometrically on the flexible, and is one of the cases illustrating good adjustment in the presence of this disjunction. Physiology was also indeterminate from this standpoint, showing mixture of traits.

To those with a perspective upon psychometric techniques, it must be clear that the conventional paper and pencil methods have for some time been moving on a plateau. No doubt it contains areas to be further explored, but there is evidence of only slightly higher ground ahead. The fixed answer, multiple choice, selective procedures have pretty well found the level at which they can function in describing the mental organization, and this is some distance below the highest to be observed. In Thorndike's (**22**) terms they can show speed and range, but they do not reach altitude. For this function it is requisite to develop procedures that are not selective but creative, where the response is autogenous, constructed by the subject himself out of a wide range of material available to his consciousness. Some intellectual fascist may counter that most minds to be measured are properly of multiple choice level anyway, and that attempts to elicit free and uncontrolled ideation of whatever sort are irrelevant if not subversive. But aside from replying that in any case it takes live ones to bury the dead, there is in the entire local culture no more cherished principle than the right of everyone to maximize his abilities. And everyone who has occasion to search for maximal abilities should realize that these are not disclosed in any selective system of measurements, and can be disclosed only under systems of creative, autogenous response.

Some findings already mentioned on psychometric patterns in high and low earning groups, afforded a slight illustration of this view. The apparent lack of objectivity, and

the resistance to quantitative treatment, have been formidable barriers to developing methods for autogenous response. But where there is concern for individuals in the upper brackets of intellectual functioning, they cannot properly be disregarded. The techniques as such are mostly very simple. The best known example is the Rorschach procedure, though it is organized from a different standpoint, and is not a very practical member of a psychometric *Gestalt.* Such membership is a *sine qua non* of effective work in the upper brackets. For essentially intellectual processes, mere word definitions can serve very well, as Wechsler has pointed out. Suitably framed questions may go deeper, and be given an evaluative or even thematic slant. As a Binet test was once translated into multiple choice, so much of the present multiple choice material can be modified for autogenous response; attitude questionnaires for example. Though again differently conceived, Street's (**20**) Gestalt Completion figures can do good, if elementary, service in the field of quasi-pictorial stimuli; and alternates in some respects an improvement on the Rorschach figures, are readily constructed.

One of the ironies of psychometric history is that Binet, Yerkes, Terman and who knows how many others, *quorum pars parvula fui*, had their fingers half closed around one of the most significant of these procedures, and let it slip through them; I perhaps with as little excuse as any, for having had the better opportunities to observe it clinically. There are some published reports of schizophrenic response to the Stanford-Binet pictures that should have meant a thematic apperception test back in 1919, and probably would have meant it if psychoanalytic insights had been better assimilated. L. A. Schwartz (**19**), in 1932, achieved a good grasp of the implications. We shall probably never know to what extent these insights helped to raise the inkblot-imagination figures to the level of a Rorschach test, but we may be sure that H. A. Murray (**17**) would acknowledge considerable debt to them in raising the Binet pictures to a thematic apperception technique, and to the range of projection methods generally; the very name of these is of psychoanalytic derivation. Their

main interest of course has been qualitative and non-intellectual.

There has been opportunity to observe a small series of students whose academic work had been notably better than there was sound reason to expect. The explanations seemed to fall into two classes. First, where there is extremely good personality organization, you may find educational as well as social adjustments made for which the average personality requires a higher degree of psychometric intelligence. I have seen this principle in operation from school for defectives to graduate professions. Second, and more relevant to the present topic, selective and autogenous responses often show marked disjunction, and cases are not infrequent where autogenous responses run well ahead of selective. Such individuals make a showing in conventional intelligence, scholastic aptitude, and short-answer examinations notably inferior to what they do in essay, creative, autogenous tasks, and will be found doing better than expectation in so far as the educational environment stimulates to original mental effort. An opposite situation, disproportionate ability in the selective functions, was illustrated in the second of the three cases just described.

Before closing it may be appropriate to allude somewhat to a broader outlook from the field of mental measurement. Many years ago, William James propounded a 'psychologist's fallacy,' that is to say a psychologizing fallacy, the confusion of one's own state of mind with the ideas about which one is thinking. An aspect of it with much social significance is the phenomenon of 'projection,' mentioned a little above. It is not absent from the history of intelligence measurement. We conceived intelligence tests in essentially ideational, even verbal, terms; terms in which we ourselves are specialists. We overestimated the degree of this trait in the population, somewhat in the 1916 Stanford-Binet Scale, to still greater extent in the organization of Army Alpha, likewise in the high mental ages that we used to assign for the upper limits of feeble-mindedness. A cultural significance that clinical psychometrics shares with its nearest kin discipline of dynamic

psychology, is a capacity to correct this particular 'psychologizing fallacy' in a measure that other disciplines do not easily reach. For how often does one see the social philosophy of social or physical scientist based upon the tacit assumption that normal humanity are paper-bound editions of his own kind? It is apparently this same projective mechanism that gives popular status and currency to the naive aphorism that 'it takes all kinds of people to make a world.' Because the 'all kinds' that are envisaged under such a remark are actually far from all kinds, but only those kinds that filter through the screen of a limited personal imagination and experience. The astronomer's eye at the telescope is now replaced by the camera and spectrograph. Nor should the mind's eye expect without comparable aids to have other than partial, even distorted vision of what 'all kinds' denotes, and moreover of the sort of world that all kinds of people make.

As the university is a function of its faculty, and the college is a function of its students, so any social system is a function of its constituents, and is very imperfectly understood in terms of institutional patterns, in detachment from the traits of individuals who must adjust to these patterns. Many difficulties with the social architecture of recent years come from the planners' attending so much to composing blueprints and so little to strength of materials. A government can often give bread to the hungry, but it can never give liberty to the suggestible, or security to the obsessive.

Like marriage, popular government is not the achievement of a solution, but the acceptance of a challenge. Yet the 19th century witnessed a widespread belief that free institutions did represent a norm which, once well exemplified, would diffuse throughout humanity on an enduring basis. There were reminders of eternal vigilance as the price of liberty, but they were more quoted than comprehended. Of late, perhaps not unaided by lessons from contemporary events, there have arisen real insights that in political patterns as in agricultural ones, the prized harvest depends on unremitting cultivation, defense against parasites, even thinnings here and there. Says the old German proverb, *Unkraut*

vergeht nicht; in a truck-garden environment the weeds are the norm, and no garden weeds itself. So it is the unmistakable teaching of history that *in the existing distribution of human traits*, authoritarianism is the norm; that the free institutions so prized in our culture depend on rather special conditions and flourish only as these are met. Already the popular press has observed that "liberty and equality are enemies rather than friends"; and so they are, when considered as political equality and economic liberty, in the presence of existing psychological differentials.

What light does the study of individual differences in relation to social adjustment throw on these matters? I shall attempt only to clarify a position which has been from one quarter complimented as a satire on totalitarianism, from another reproached as "virulently anti-democratic." The argument runs somewhat as follows:

The libertarian régime connotes status by achievement: the authoritarian, status by ascription (in terms borrowed from Professor Lynd, **15**). Neither is immune to patterns of exploit—the 'pecking order' of human relations. In the ascription régime the high-born have peck-right over the base-born. In the achievement régime the knowing can peck the simple. Either scheme is workable, in contrast to doctrinaire socialism, in which the dull are to exploit the alert. Though apparently suited to a gallinaceous society, the role of pecking orders in a democratic ideal is highly questionable. If current authoritarianism were swept from the earth, popular government would still have the basic problem of achieving a viable organization on a basis of mutuality rather than exploit, and developing adequate defenses against the neurotic will to power.

Ideas about education for democracy are generally based on redirections of the pecking impulse. There are advantages also in a standpoint which largely accepts the pecking impulse for what it is worth, but looks towards a community whose members are sufficiently close together in qualifications for the peck-right, that these tensional releases are determined by choice rather than by status, and by exchange rather than by exploit.

While the immediate problem of democracy is to stand off the totalitarians, its ultimate one remains that of reconciling freedom and integration. In contrast to the social insects, the normal way for human intelligence to achieve an integrated society has been through authority, and the pious hope for a 'coming victory of democracy' any less superficial than the last one, depends on finding ways to better integrations on a basis of liberty than can be had through the authoritarian norm. General, secular trends towards closer social coordinations, regimented or otherwise, are fairly obvious, and seem on technological grounds ineluctable. At the moment, the most effective enemies of free institutions are the internal combustion motor and the thermionic valve.

The pecking hierarchy of the barnyard is a natural and direct function of differential aggressiveness. Wide differentials in human abilities of various sorts are similarly adapted to patterns of exploit, and by this token to the authoritarian pattern, and to patterns of libertarian over-competition. The mutual aid or cooperative or fraternal ideal must be rather the magnification, to industry-wide and state-wide proportions, of the social system represented in the good amateur exploring party or sports team. Where these are normally organized, both opportunity and inclination to exploit are minimal and any needed leadership has a very strict group sanction. A special characteristic of these small but very efficient social systems is limited tolerance for deviations from a norm of effectiveness. For minus deviations this is obvious enough; the persistent loser of football yardage, the doryman who never catches his share, are readily eliminated from their respective groups. The super-competent are less likely to be eliminated but rather may, prescriptively or otherwise, acquire a status of authority in which the fraternal functioning of the system is obscured. As those who achieve consolidate their gains for themselves and those about them, an achievement régime always goes over into one of ascription. William Hard has remarked, "If we are none of us to be dominators, then we all of us have to be cooperating equals." Abstracted from psychological equality these words mean little; subsuming it, they mean everything.

You can be a D student in History 1 and stay in the course longer than you can be a D football player and stay with the squad. A basically authoritarian system can be more tolerant of large differentials, as it can more readily allocate a functional role most serviceable to, or least interfering with, the leader or the group. Reference has been elsewhere made to analogies subsisting in this respect, between the totalitarian polity, and such shining examples of authoritarianism at its best, as are found at Vineland, N. J.; at Letchworth Village, N. Y., or at Wrentham, Mass.

On all these points, be it thoroughly understood that quantitative rather than qualitative differentials are concerned. In a libertarian henhouse, it is all to the good that the inhabitants should have varying directions for their pecking impulses. But this freedom is largely nullified when they differ extremely in their capacity to give, receive or reject such attentions. So also, it is good for human freedoms that different people should want different things. It is not good that they should differ as they do in their several equipments to get what they want.

Some fancy the social goal as a luxuriant jungle of competitive individualism. This outlook should make special appeal to personality types of the lion, leopard, bushmaster and weasel classes. Others see it as a zoological park, well-kept under benevolent direction—with themselves as curators. This should have special advantages and attractions for the human elephant, giraffe, sheep, prairie-dog. A pattern more distinctive of *homo sapiens* would be—it is still early to say would have been—a society of individuals adapted to self-sustenance in a medium of intelligent mutual endeavor and trust. The human personality is fully capable of this; but the continued maximizing of its differentials, whether through education or genetics, is a doubtful way to achieve it.

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CRITERIA OF FRUSTRATION

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Orientation

The investigation of *frustration* as a key to the problems of normal as well as abnormal motivation has been essayed on a number of fronts recently, although its significance was already implicit in the work of G. Stanley Hall (*e.g.*, **35**) and of Pavlov (**64**, *cf.* **52**).

The fruitfulness of the work on this problem is seen in many experimental studies of the past five or six years, including among others those of Anderson (**1**), Barker, Dembo, and Lewin (**4**), Clites (**11**), Curtis (**15**), Haslerud (**38**), Karn (**43**), Liddell (**52**), Maier (**55**), Mowrer (**63**), and Rosenzweig (**66**). From a theoretical standpoint, frustration has been heralded "as a technique for investigating the nature of the intellectual abilities" (**3**, p. 150); as replete with physiological, as well as psychological, implications (**70**); and as a factor in lessening the "artificial division which now separates normal from abnormal psychology, and abnormal psychology from theoretical psychiatry" (**71**, p. 160). Nevertheless, caution should be exercised in adopting without qualification certain generalizations such as, "creation or growth and frustration or thwarting are merely two aspects of the same life process" (**70**, p. 127).

It would be difficult at the present time to postulate a definition of frustration that would find ready acceptance among all investigators. A number of definitions have been advanced, each seizing upon some essential elements of the frustration process to the exclusion of other equally important aspects (*e.g.*, **3**, **21**, **71**). However, these will usually be found to have more in common than may at first appear to be the

[1] The writers are indebted to Dr. O. H. Mowrer for many critical suggestions.

case, with any sins being those of omission. Some criteria of frustration, therefore, may be established even now, albeit tentatively. That is to say, certain limited aspects of frustration may be offered to which all judgments relating to the problem can be referred, of course with the understanding that any gaps for which data are as yet wanting may be filled in from time to time as research progresses. Such a presentation is the aim of this paper.

Interrelation with Other Psychological Processes

For the understanding of the frustration process in its proper context, clarification is to be sought in its relation to other activities, rather than in any predilection for conventional labels or classification. To assign frustration a place on one of the psychological 'shelves' would still leave us with the responsibility of saying why the concept is so shelved. Moreover, some will seriously question any premature identification of frustration with emotion, for example, arguing that emotion is more appropriately applied to a reaction to frustration, *i.e.*, anger.[2] The usefulness and validity of any other identification might, similarly, be taken to task. Nevertheless, the frustration process may be conceived of in entirety as having *aspects* of emotion, tension, conflict, inhibition, aggression, withdrawal, regression, adjustment, and the like, whose proper relations to frustration constitute a real problem.

What are the emotional aspects of frustration? It is difficult to treat frustration very precisely in relation to emotions, considering the divergent and even conflicting views of emotion in the history of psychological thought.[3] However, in the laboratory human activities such as anger, fear, disgust, joy, and sorrow are unequivocally dealt with as emotions (*e.g.*, **60**). Typically an emotion of this kind "is aroused by external stimuli and is directed toward the environment" (**82**, p. 234). Moreover, Cannon has demonstrated unique organic states in the case of fear and anger

[2] O. H. Mowrer, personal communication to one of the writers.

[3] *Cf.* **2**; **5**; **10**; **18**; **20**; **32**; **34**, pp. 245–6; **35**; **42**; **49**; **50**; **51**; **54**, p. 242; **64**; **78**; **79**; **82**.

(*cf.* **8**, **9**). Obviously if frustration is *causal* in its manifestation, it is related to emotion as stimulus is to response; if it is *effect*, it is then possible to show that it may also be aroused by external stimuli and directed toward the environment. These two concepts are not incompatible; the notion of frustration either as cause or as effect is equally tenable, provided that frustration is in turn related to its proper context. Thus, to speak of a frustrating situation or external obstacle as does Mowrer (**63**, p. 68) in no way negates the proposition that the organism *per se* may be frustrated, *i.e.*, suffer the obstruction of its activity. Differences of opinion of this sort are often relative (*cf.* **40**).

Almost fifty years ago Dewey (**20**) pointed out the close connection between emotion and human activity, advancing the hypothesis that emotion appears when human activity is obstructed. More recently Kantor (**42**) has even contended that an emotion corresponds to an actual breakdown in behavior, while Cannon and Pavlov "directed more attention to the affect than to the disorganization of the behavior" (**54**, p. 12). It should be emphasized, however, that Pavlov showed that affect is the result of a definite disturbance in the behavior of the animal; in many instances he obtained some very definite affective 'breaks' (**64**). Apparently these were disturbances in the higher nervous activity of functional origin, and not "due to surgical interference or trauma" (p. 284). An assignment to different, mutually exclusive categories of the frustrating situation and the frustrated organism, or of the frustrated organism and the corresponding emotional components, would be extremely artificial. This is a special case of Dewey's refusal, generally, to admit any such disjunction between stimulus, response, and effect. For purposes of analysis only we may speak of the frustrating process as constituting (i) a frustrating situation involving conflict or inhibition; (ii) a frustrated organism characterized by change in tension, disturbance of homeostasis (*cf.* **8**, **9**), and maladaptation; and (iii) a frustrated reactional system involving emotional reactions such as anger, primitive reactions like aggression, as well as with-

drawal, regression, or even complete breakdown.[4] Frustration cannot be defined by either the stimuli or the responses considered separately, but rather by the interaction of these two systems. The gross behavior result or, to paraphrase Tolman (**78**), the nature of the back-action of the responses upon the stimuli, is important.

Analysis of the Frustrating Situation

(*a*) *The element of barrier or obstruction.*—Mowrer (**61**, **62**) has emphasized the operation of a barrier in the frustration sequence whence conflict or inhibition may ensue. Clearly such a barrier need not necessarily be real or apparent, as in the case of the rat faced with an electrified grid, but may be quite imaginary or arise out of anticipation of punishment, injury, or the like, as the fear aroused through a warning or danger signal.

> To take a simple example, an adult may say to a child, "Don't do that." If the child is on the verge of executing the interdicted act, then the fear aroused by the warning acts as a barrier, in this case mainly internalized. One of two things usually happens. The child will heed the warning (withdraw, desist) or it will over-ride the barrier and continue the contemplated behavior. The determining factor here is, I think, a purely quantitative one. *How much* anxiety does the warning arouse? If it arouses much anxiety, flight will ensue. If it arouses little, it will be counteracted or canceled by aggressive impulses and the original course of action will go forward unchanged, or perhaps even more determined and energetic than it would have been without the warning.[5]

While the external object need not be insurmountable (**61**, p. 68), sufficient pain is associated with it, however, to stimulate "psychological locomotion away from the region of the frustration" (**3**, p. 149). The thwarting or hindrance (terms used synonymously with frustration, *e.g.*, **3**, pp. 145–6) which the barrier constitutes is not limited to the individual's

[4] These related aspects of frustration will be discussed in the three following sections.

[5] Mowrer, *op. cit.*, footnote No. 2.

activity in progress, to what he needs or is accustomed to, but also relates to that which he is expecting or to which he is looking forward. "Some of life's keenest disappointments arise from failure of expectations and hopes" (**60**, p. 84; *cf.* 56).

(*b*) *The factors of goal-attainment, and reward-expectation.*— Dollard *et al.* have attributed frustration to the "interference with the occurrence of an instigated goal-response at its proper time in the behavior sequence" (**21**, p. 7). Concerned with the rôle of frustration in human affairs, 'goal-response' is defined as "an act which terminates a predicted sequence" (**21**, p. 6). Similarly, G. L. Freeman (**25**, p. 226) uses the phrase 'conflict situation' in describing a state of affairs where "an inhibiting condition of frustration to the realization of the goal" is likewise exhibited. More generally, frustration may be aroused by upsetting the 'postural tension' or "the general bodily background (neuro-glandular-muscular) operating to sustain and energize the overt response pattern at any given moment" (**25**, p. 226). Thus, it would seem that frustration occurs when there is interference with goal-attainment. In the experimentally induced situation of conflict, "some basic tissue condition is prevented from attaining its usual and immediate channel of expression by the presence of other stimulation, and bodily changes are observed before, during, and after removal of the thwarting condition" (**25**, p. 229). In general terms also Haslerud (**38**, p. 136) suggests that frustration can be "experimentally manipulated if one is able to change the goal tendencies so that various degrees of negativity to a situation replace or conflict with the previous positive tendencies." In this connection, Curtis (**15**, p. 144) appropriately cautions that "a more adequate sampling of the many processes of the body [may] reveal deep-seated disharmonies fundamental to an experimental understanding of the conflict problem."

Interference with attainment of reward, desire, or need represents a special case of disturbance of the goal-response. Thus, Haslerud (**38**) working with chimpanzees found experimentally that interference with a reward-expectation was

frustrating. As a matter of experimental fact, moreover, Maier (**55, 56**) and others (**16, 43, 46, 54**), particularly Liddell and Bayne (**53**) as well as Anrep and Pavlov (**64**), have produced behavior disturbances strikingly resembling neurotic behavior by restricting their subjects' responses, or by enforcing a situation designed to require discrimination between two similar stimuli provocative of antagonistic behavior. Cook (**14**) has summarized the principal methods used to produce 'experimental neurosis' (*cf.* **12, 13**), and specific techniques for inducing frustration (or neurosis) have also been described by Haslerud (**38**), Anderson and Liddell (**1**), Luria (**54**), and Maier (**55, 56**).

White (**81**) emphasized that frustration cannot occur at the vegetative level of response, *e.g.*, the heartbeat. At the 'psychological level,' however, doubt enters into a situation demanding that a choice be effected between or among several alternative courses of action, and frustration may occur. This subjective aspect is mentioned, too, by Barker (**3**) who speaks of a frustrating situation as any "in which an obstacle —physical, social, or conceptual, personal or environmental— prevents the satisfaction of a desire. The subject himself accepts the obstacle as impassable, the solution as impossible" (**3**, p. 146; *cf.* **4**). In his discussion of the reduction in the cognitive level of behavior which may occur in frustration (*cf.* **77**), Barker (**3**, p. 147) also analyzes frustration as involving "a conflict for the control of the organization of the perceptual-cognitive-motor functions between the forces corresponding to the frustrated need, on the one hand, and the forces corresponding to needs which may be satisfied in reality, on the other."

This element of conflict has been emphasized by Rosenzweig in connection with privation and deprivation. He points to frustrated attempts at satisfying such needs as 'positive biological processes of defense,' nutrition, sexual activity, and social relationships. From these elements, *i.e.*, 'conflict,' 'deprivation,' and 'privation,' Rosenzweig has constructed six 'types' of behavioral disturbance, each of which in turn he believes may be aroused either externally

or internally (**70**, p. 151). His examples of these 'types' are consistent with his systematic treatment of frustration to which he has devoted a number of papers, experimental and theoretical (*e.g.*, **65**, **67**, **69**). Thus, he describes the 'destitute' as a case of external privation or deprivation; the 'delinquent' as illustrative of external conflict; the 'defective' as indicative of internal privation or deprivation; and the 'disordered' as a case of internal conflict. The rigor of Rosenzweig's 'types' will depend upon findings which may result from more clinical work. Few psychologists would maintain that these classes are logically and psychobiologically exhaustive.

Analysis of the Frustrated Organism

Having examined the nature of the barrier and of the interference with goal-attainment and reward-expectation which together constitute the frustrating situation, we may now turn to the effects on the organism experiencing the frustration. These effects, which may be isolated for convenience of discussion only, include (*a*) failure, (*b*) change in tension, (*c*) cognitive effects, and (*d*) frustration-tolerance. We should also consider the cultural implications of these factors.

(*a*) *Failure.*—The conflict and inhibition which ensue from the frustrating situation have direct bearing upon the organism's "failure in satisfying certain needs of the ego" (**67**, p. 398), or, more broadly, its "failure in biological adjustment" (**71**, p. 151). White (**81**) described frustrated efforts as those which lack certainty and have a high percentage of failure (*cf.* **7**). Failure eventuates because "the flow from stimulus to action is not so direct but on the contrary is intercepted by numerous possibilities which tend to prevent, frustrate expression in immediate action" (**81**, p. 153; *cf.* **6**, **72**). Implied in this statement is the rôle of conflict, essentially the same as that ascribed by Dewey (**20**) to emotion. An "inevitable tension, interference, mutual checking, friction, struggle, competition, or conflict (terms used synonymously by Dewey) between the pertinent and non-pertinent or aborted features of the total response is the immediate condi-

tion of the 'emotional seizure'" (**2**, pp. 391–2). Thus, while James had previously denied any crucial function to conflict as a condition of emotion, such conflict became for Dewey its basic condition. Apart from the question as to the definitive relation of conflict to emotion—whether it be constitutive, incidental, or only a result (**2**, p. 401)—its rôle in the frustration process is unequivocal, to wit: It ensues from the frustrating stiuation, and has the effect of producing an emotional reaction, quite frequently identified as anger. Thus emotions arise when reactions that cannot be readily absorbed "resist ready integration with those already in orderly progress towards fruition. Such resistance means actual tension, checking, interference, inhibition, or conflict—conflict between the intercurrent reactions appropriate to the original 'set' and reactions of other origin threatening the abortion or frustration of the relevant series" (**2**, p. 401).

(*b*) *Change in tension.*—The association of tension with frustration causes confusion unless the meaning of 'tension' is clearly defined. Yet 'tension' is certainly not used in the same sense by all investigators, *e.g.*, Stroud (**76**), Mowrer (**62**), Shaffer (**73**), and Freeman (**24**, **25**). In speaking of 'anticipatory tension or preparatory set' (**62**, p. 74), Mowrer asks whether we can justifiably assume that psychological tension varies directly with muscular activity, or whether muscular activity causes the dissipation of tension. In his opinion we would not be justified in equating tension simply to muscular tonicity, for he thinks of tension primarily as a 'central phenomenon.'[6] Since anxiety can be thought of as anticipated pain associated with a rise of tension—as pleasure is with tension reduction (*cf.* **28**)—the frustrated organism experiences a rise in tension in the above sense, according to Mowrer. Rosenzweig, for whom frustration may involve states ranging all the way "from mild craving or unconsummated drive to more acute interference with satisfaction by thwarting, to sudden and extremely shocking or traumatic experiences" (**70**, p. 126), also associates a rise in tension with frustration "when interest is centered upon the state of the organism"

[6] Mower, *op. cit.*, footnote No. 2.

(**70**, p. 127), as opposed to the "environmental factors with which the organism is failing to cope successfully." To this type of frustration he applies the term 'maladaptation.'

If 'tension' is used in the sense of muscular tonicity, then a rise in tension is not inevitable in a frustrating situation, as Freeman (**25**) points out. Certainly "'successful' solutions of arithmetic problems by normal subjects have been accompanied by greater [**11**] or the same [**19**] amount of muscular activity as have reported 'failure' reactions, where we have expected greater tensions due to frustration" (**25**, p. 228; *cf.* **26**). Freeman suggests a limited range of postural activity (*cf.* **74**) by which effective 'phasic adjustment' is sustained; and, he continues, "if tension levels exceed these limits, breakdown and disorganization of the higher order response occurs" (**25**, p. 237).

White considered doubt to be the subjective state of what may be described "as a state of tension resulting from the frustration of stimulus the energy from which is prevented, for the time being, from translating itself into action. When action takes place the tension is relieved and the feeling of doubt is replaced by one of satisfaction" (**81**, p. 159). Apparently White also implied an increase in tension as a direct outcome of the frustrating situation; in this respect, frustration (tension-increase) is the antithesis of happiness (tension-decrease).

(*c*) *Cognitive effects.*—In discussing the cognitive effects of frustration, Barker (**3**, *cf.* **75**, **77**) acknowledges the emphasis usually placed on the ability to overcome the difficulty from which frustration arises. He asks what the effect is "of a frustrated need upon the intellectual level of behavior not directly related to the satisfaction of that need" (**3**, p. 145). There are two opposing points of view in this regard; on the one hand, frustration may be harmful to intellectual behavior (*cf.* **81**); on the other hand, frustration might be considered as favorable to creative behavior of a high order. Believing that frustration in general results in a change in cognitive ability, Barker is willing to accept the possibility that actions occurring in a frustration context are on a lower creative level

than similar actions in a context of non-frustrated activities, but he points to certain experimentally established cases of an increased level of cognitive functioning resulting from frustration, *viz.:* (i) When the "introduction of a frustration markedly raises the cognitive level of a neutral activity" in a lethargic individual. (ii) "When the neutral action becomes a substitute for the frustrated one" (sublimation). (iii) When the reaction is "characterized by an escape into reality, a losing of oneself in a neutral task" (**3**, p. 149).

At a time when little attention had been given by psychologists to frustration *per se*, G. Stanley Hall (**37**) discussed sublimation as associated with anger and its *Abreagierung* (*cf.* **33**). Recently LaBarre has analyzed hundreds of limericks as "probably the peculiar property of sexually frustrated but healthy and normal young men" (**48**, p. 204). The rôle of cognitive effects in frustration is very significant in the interplay of culture and behavior.

(*d*) *Frustration-tolerance.*—It has been reported in studies of animal behavior that the animal may ignore the food stimulus when the interfering barrier yields pain in excess of that which it is presumably willing to tolerate. Brunswik presented rats with a single choice situation and sought to determine to what extent they could distinguish a better chance of reward from a less good one. He found that there was an increase in learning with an increase of the probability ratio of punishment. "Apparently the avoidance of punishment becomes a stronger motive as compared with the anxiousness of the animal to reach food as quickly as possible" (**7**, p. 185). This finding is suggestive of possibilities for investigations of frustration-tolerance with human and infrahuman subjects. Certainly frustration may ultimately result in tolerance whenever the animal, under the influence of punishment-anticipation, either ignores the frustrating stimulus without suffering any untoward effects, or substitutes responses more satisfactory to the organism (*cf.* **21**).

In this connection, Rosenzweig concludes on the basis of his own experimental work (**65**, **68**) and that of others (*e.g.*, **44**) that the degree of frustration-tolerance may be useful in

distinguishing between the psychotic, neurotic, and normal individual "in whom a relatively high frustration-tolerance would usually be found throughout the personality" (**71**, p. 153). This concept has real significance for the cultural transmission of behavior. Thus, Mowrer (**61**) has discussed the consequences of frustration as implementing and facilitating the demands of a particular culture upon an individual, in the process whereby he achieves various adjustments to the cultural patterns (*cf.* **39**). Such a process is one of serious implications; indeed, the imposition of frustration under pressure "of innumerable deprivations, thwartings, and penalties which may be gross or subtle" may be indispensable to education (**61**, p. 130), provided a tolerance is built up (*cf.* **23**). Freud (**27**, p. 310) emphasized the risk which is inevitable in any education involving neurotic children upon whom 'the frustration of reality' has had evil effects.

Specifically, Mowrer (**61**) distinguishes two categories of frustration effects upon the individual, to wit, 'habit progression,' which is non-pathogenic and which may lead to the acquisition of a new mode of adjustment; and 'habit regression,' which may be pathogenic and may lead to a previously abandoned mode of adjustment (*cf.* **4**, **80**). Obviously frustration-tolerance is involved only in the first of the above-mentioned categories. In the complete absence of this tolerance, frustration may result in changed behavior enduring for life, as Curtis (**15**) points out in the case of sheep. Mowrer suggests that human frustration corresponds to two major aspects of culture, *viz.*, "the transmission from generation to generation of useful techniques and skills and the perpetuation and enforcement of regulations and codes governing social conduct" (**61**, p. 133). Pertinent in this regard is the problem of the manifestation of frustration in competitive as opposed to coöperative cultures (**58**, **59**). In some societies, too, the regulation of conduct apparently involves no conflict at all; tradition determines one's behavior so that there are no momentous decisions to be made (**45**).

Frustration-tolerance is implied also in Freeman's sug-

gestion that frustration in man is a psycho-physiological matter, involving 'standards of achievement,' 'set,' and 'levels of aspiration.' Elaborating this thesis, he states that the "frustration involved in the inhibition or 'thwarting' of a basic organic disposition is largely a matter of how the subject relates his present performance to past behavior—his own and that of others around him" (**24**, p. 229; *cf.* **62**). This is suggestive, in turn, of the effect upon the individual of the so-called 'cultural privations' of Freud (**29**, p. 59), who distinguished between the individual, recognizing no restriction, and the members of a community, mutually restricting their 'possibilities of gratification.' Of these restrictions, he added elsewhere that "the limitation of aggression is the first and perhaps the hardest sacrifice which society demands from each individual" (**31**, p. 151). Again, it must be emphasized that impulses to aggression are probably not universal (**45**), although Freud's remarks perhaps apply to our own culture. As an example of cultural conditioning in frustration, Freud mentioned the sexual barriers in the way of the adolescents in our culture, because we assume that marriage is socially inexpedient for such youthful individuals. Finally, there are remarkable deviations in frustration-tolerance. This is demonstrated in Dabrowski's (**17**) study of self-torture as a means of alleviating more painful mental anguish, in which the cultural values are amply discussed (*cf.* **34**, **54**).

Analysis of the Reactional System in Frustration

Categories such as learned and unlearned, primal and conditioned, subjective and objective, have been employed by various writers. However, the categories to be discussed below of the various reactions to frustration are not to be regarded as 'air-tight' classifications but simply as convenient ways of treating this problem Although some of this material is closely related to the section above dealing with the effects of frustration, it is included here as part of a summary of objective and subjective laboratory findings.

(*a*) *Objective reactions to frustration.*—(i) *Aggression:* Mowrer says: "Aggression, in its primitive, uninhibited form,

consists of an *energization* of the behavior in progress, which has the distinct biological utility of tending to overcome the frustrating barrier and thereby clearing the path to the goal" [7] (*cf.* **21**). As such, he believes it to be the natural, unlearned reaction to frustration, as contrasted with withdrawal which is a learned or conditioned reaction "based on the experience of aggression having been followed by punishment." The authors of the frustration-aggression hypothesis have defined both terms objectively, and have assumed "that overt, direct aggression against the agent which is perceived as the source of frustration is most satisfying to the organism, *i.e.*, is the response to which there is strongest instigation" (**21**, p. 48). Doob and Sears have written of "unique changes within the organism which serve as instigation to aggressive behavior" (**22**, p. 293), but one wonders what resemblance these changes have, if any, to the physiological changes in emotion. They postulate a 'non-overt aggression' wherein frustration is "followed not only by aggression which may vary in the degree to which it is expressed overtly, but also by substitutive responses as well" (**22**, p. 294). They admit, moreover, that "in many frustrating situations . . . it is difficult to find evidence of either substitutive responses or overt aggression in the behavior of the frustrated person" (**22**, p. 293). Indeed, aggressiveness is unknown in certain societies, and in general aggressive behavior presents wide variations. "Conflict may be inevitable, but culture determines its mode of expression" (**45**, p. 268; *cf.* **57**). It is evident from such considerations that any contention regarding aggression as the necessary outcome of frustration does not apply to all cultures.

(ii) *Withdrawal and regression:* Withdrawal appears to be a learned reaction. That is to say, social conditioning, previous experience, or anticipation of pain or punishment causes the animal to flee the barrier rather than to surmount or attempt to surmount it. Withdrawal from reality was stressed by White as a common consequence of frustration; he described the sham-death reflex (*cf.* **47**) as "a passive form of defense from danger allied to the stupors, catalepsies, and

[7] Mowrer, *op. cit.*, footnote No. 2.

trance states, etc., in humans" (**81**, p. 154). Expressed in terms of energy, White argued that such consequences re-establish the individual's equilibrium, by reducing his 'state of tension' previously existent. Freeman also refers to tension levels which may affect the 'balance of factors,' which, in turn, "may favor 'withdrawal' from new and potential conflict situations and regression to behavior of less than appropriate energy requirement or some degree of 'over-compensation' and 'progression' to behavior characteristics of more extravagant than normal tension levels" (**25**, p. 237). What is the rôle of anxiety which is attendant upon such reactions of withdrawal? Its observable effects in infra-humans have been strikingly summarized by Curtis in his analysis of the effect in sheep of experimental neurosis produced by Anderson and Liddell (**1**):

> If training is continued, the animal passes over into an excitable phase, where conditioned reflexes are more violent than normal, and positive responses are given to stimuli that were formerly negative, discrimination between stimuli having completely broken down. During the interval of several minutes between stimulations, the animal cannot maintain its former state of alert quiet, but exhibits at frequent and irregular intervals spontaneous tic-like movements of the reaction limb which Anderson compares to the fidgeting of a nervous child. . . . This is probably the most characteristic overt manifestation of the neurosis in sheep (**15**, p. 142).

Here the behavior changes are not a regression to an earlier habit-pattern but involve the animal's 'homeostatic physiological relationships,' *e.g.*, speed and regularity of pulse and respiration. Furthermore sheep, previously coöperative, become refractory and resist laboratory treatment (*cf.* **55**, **56**). Similarly, Krasnagorski (**46**) found that frustrated children displayed an unwillingness to return to the experimental situation, and either an inability to invoke previous habits of responding to certain conditioned stimuli, on the one hand, or on the other hand of inhibiting responses to unreinforced conditioned stimuli, once these inhibitions had been

acquired. Such was the case also with some of the classic experiments of Pavlov (**64**). His dogs showed struggling and howling behavior, and finally 'broke down.'

Occasionally the frustration effect fails to take complete hold of the animal, whereupon withdrawal is not consummated. This was the case with Haslerud's chimpanzees who exhibited such reactions to frustrating situations as "hesitation at work on the task," "reluctance to resume a task after a frustrated attempt," and "various amounts of violence and emotion directed toward environmental objects or toward the animal itself" (**38**, p. 138). These animals even adapted themselves to the frustrating situation, although they varied in their resistance to it.

(*b*) *Subjective reactions to frustration.*—Rosenzweig (**67**) believes that there are three types of subjective reaction to frustration. In the first instance, the subject may show anger; condemn the outer world, *e.g.*, persons, objects or circumstances; and adopt an attitude of hostility toward his environment ('extrapunitive'). In the second place, the subject may react with 'emotions of guilt and remorse,' with a tendency to condemn himself as culpable ('intropunitive'). Finally, the subject may experience embarrassment and shame and may attempt to effect a conciliation to the disagreeable situation. "In this case ('impunitive') he will be more interested in condoning than condemning and will pass off the frustration as lightly as possible by making references, even at the price of self-deception, to unavoidable circumstances" (**67**, p. 395). Here, too, anxiety probably plays a significant rôle, *i.e.*, in determining which form of the three reaction-types above will occur; that is to say, the subject may take into consideration the counter-reaction to his own behavior, in anticipation of pain, or fear of punishment, and react accordingly. Although the author of these 'types' points to what he considers to be a similarity between them and the 'libidinal types' of Freud (**30**)—to wit, 'narcissistic' (extrapunitive), 'compulsive' (intropunitive), and 'erotic' (impunitive)—the resemblances seem obscure.

White described a 'choice type' of reaction which "permits

a consideration of a greater number of stimuli from a broader field of consciousness" (**81**, p. 153). Reactions of this type are less stereotyped than violent motor reactions which may lead the animal either to death or to safety, there being no possibility here of varying the response (*cf.* the *Bewegungssturm* of Kretschmer, **47**). Thus, in the choice type of reaction, "The flow from stimulus to action is not so direct but on the contrary is intercepted by numerous possibilities which tend to prevent" immediate activity (**81**, p. 153).

Summary

1. An attempt has been made to set up criteria of frustration through an analysis of its various aspects. Emphasis has been placed upon the experimental work of the past five or six years.

2. The frustration process seems to have aspects of emotion, tension, conflict, inhibition, aggression, withdrawal, and the like, which have been examined in terms of the studies of frustration.

3. The frustration process is delineated as consisting of a frustrating situation, a pattern of effects involving the frustrated organism, and a reactional system.

4. The frustrating situation is analyzed in terms of barrier or obstruction, and of interference with goal-attainment and of reward-expectation.

5. The factors of failure, change in tension, cognitive effects, and frustration-tolerance are attributed to the frustrated organism. The cultural implications of frustration are also considered in this connection.

6. Reactions to frustration may be aggression, withdrawal, regression, resistance, anger, guilt and remorse, shame and embarrassment.

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